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NOTES FROM THE PHYSICAL LABORATORY.

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A Note on the Effect of Pressure upon the Series in the Spectrum of an Element. By J. S. Ames and W. J. Humphreys.

It has been known for many years that the spectra of certain elements, notably the alkalies and the alkaline earths, contained series of lines, which obeyed a mathematical law like that giving the distribution of lines in the spectrum of Hydrogen. These series have been thoroughly studied by Kayser and Runge, who have classified them according to their physical characteristics as principal, first subordinate, second subordinate. Only a few elements, Lithium, Sodium, Potassium, have in their spectra all three series; while the last two series, the subordinate ones so-called, are common to some ten others; the wave-lengths being different in the spectra of the different elements, the physical properties being the same.

While the shift of the lines in the spectra of the elements was under investigation, it seemed important to study in particular the effect of pressure upon the series of the various elements. To this end, photographs were taken of the arc-spectra of all elements which give series, both at ordinary pressure and at increased pressure; and the shifts were carefully measured of as many of the lines as possible. In certain cases eye-observations were also made. The results for each element may be thus briefly stated:

1. The lines of any one series of a particular element are shifted alike, *i. e.*, according to the same law, which may be written:

$$\Delta\lambda = \lambda\beta\,(p_1 - p_0)$$

where λ is the wave-length, $\Delta\lambda$ is the shift produced by an increase of pressure $p_1 - p_0$, β is a *constant* for any one series of a definite element.

2. The constant β is different for the different series of the same element, the change being such that, *very nearly*, β for the principal series is one-half β for the first subordinate and one-quarter that of the second subordinate.

3. The constant β is different for the same series of different elements. Special attention is called to this fact in another note in this Circular. One apparent regularity which demands attention is the fact that, approximately, the value of β for similar elements (*e. g.*, zinc, cadmium, mercury), varies as the cube-root of the atomic weight.

No satisfactory theory has been advanced to account for these shifts of the spectrum lines when the arc is under pressure. There is every evidence that it is not due to a temperature effect of any ordinary kind. It would certainly be expected that the outer envelope of an arc would be at a much lower temperature than the core, and that different electric currents might cause different temperatures in the arc; yet no shift due to these variations have been observed. It is our intention to observe the arc under these conditions with a Michelson refractometer as soon as possible and so to learn what actually occurs. Again, the temperature of the arc should be much greater than that of a Bunsen flame which is being fed with sodium; but the difference in the wave-length of D_1 under the two conditions is not perceptible with a 21 ft. concave grating 1[illegible] 000 lines to the inch: *i. e.*, a shift, if any, must be less than 0.002 of an Angström unit. The words "temperature of the arc" are used with considerable hesitation, because so little is known as to the mean condition of the molecules of the vapor which are producing the light.

One can easily understand, however, that, if the pressure on a gas is increased, the number of collisions per second must increase; and it is not impossible that this increased internal energy of a molecule, as thus produced, is the immediate cause of a change in the actual size of the molecule. The extent of this change would depend upon the looseness of construction of the molecule, apparently: and this quantity is measured to a certain degree by the coefficient of expansion of the element in the solid form. Therefore, it might be expected that the measured shift would vary in the same direction as the coefficient of expansion of the solid. This is actually the case, with no exception. Again, since on any theory of emission of waves the wave length varies directly as the [illegible] dimension of the portion of matter producing the waves, it would be expected that the measured shift would vary directly as the coefficient of *linear* expansion, which is found to be the case. If α is this coefficient, it may be stated as an experimental law that, for different elements $\beta = c\alpha$ where c is a constant, which in some way connects the ordinary changes in size of a solid due to temperature changes with the hypothetical changes in the size of the molecules of the vapor due to pressure changes.

It is not difficult to see that one would expect, as consequences of the above ideas, that the shift would be proportional to the total increase of pressure, regardless of its mode of production; and that it should also vary directly as the wave length in any one series or group of lines, for in such a case the longer waves indicate greater linear dimensions of the vibrating segments, if the term may be used. These are observed phenomena, as is stated above.

The fact that the shift characteristic of a principal series is less than that of the first subordinate, and this in turn less than that of the second subordinate would be expected, in accordance with these ideas, *if* the molecules producing the principal series were of a simpler structure than those producing the first subsidiary; and if the molecules producing the second subsidiary were the most complex of all. For, since the shift of a series depends upon the looseness of the molecular structure, it would be expected that, if these molecules split up in any way, the fragments would be more stable and firm than the original molecules, and therefore the shift of the original molecules would be greater than that of the fragments. It is difficult, however, to see any reason why the shift of the different series should vary according to any simple law, or why the shift of the same series of different elements should be in accordance with any formula so simple as that of the cube root of the atomic weight. If it could be assumed that the shift was proportional to the linear dimensions of the segment producing the waves, most interesting deductions might be drawn; but there seems to be no justification for the assumption.

A Comparison of Rowland's Mercury Thermometers with a Griffiths Platinum Thermometer. By C. W. Waidner and F. Mallory.

The determinations of the mechanical equivalent of heat by electrical and mechanical methods show a close agreement between the values obtained by different experimenters using the same method of experiment. There appears, however, a very appreciable difference between the values as determined by the two methods, a difference greater than can be accounted for by errors of experiment. This difference can only be explained on the assumption that the results of the different experimenters are based on different thermometric standards or of a still undiscovered error in the system of electric units employed. The recent comparison by Professor Schuster of Joule's thermometers has indirectly furnished a connection between Professor Rowland's air thermometer and the nitrogen standard of the Bureau International.

This comparison pointed to differences in these two standards as great as .05° C., but as the details of Joule's comparison of Rowland's thermometer (No. 6166) with his own are not known, this correction is uncertain. It was therefore thought advisable to make another comparison of Professor Rowland's mercurial thermometers *under conditions* as nearly as possible *similar* to those under which they *were used* by him in his mechanical equivalent determinations. On account of the many advantages offered by a Griffiths platinum thermometer for the standardization of calorimetric thermometers, this instrument was selected for the comparison. In the comparison the platinum and the mercurial thermometers were placed side by side in a calorimeter and the two read simultaneously while the temperature was slowly rising. Readings on mercurial thermometers were taken by means of a micrometer eye-piece.

The stem corrections of the mercurial thermometers were made, as in Rowland's experiment, by surrounding the thermometer stem for a short distance above the calorimeter with a water jacket, the remaining portion of the exposed stem being assumed to be at the temperature of the surrounding air. The mercury thermometers were placed in ice and distilled water for several hours and the zero determined before each comparison. The corrected stem readings obtained in our comparison were then reduced to Rowland's zero and the corresponding temperatures of his air thermometer taken from his tables of comparison of this thermometer with his Baudin thermometers (Nos. 6163, 6166). The platinum thermometer used in this experiment was one constructed by Mr. E. H. Griffiths. It was provided with compensating leads so that the temperature was independent of stem immersion, provided this was great enough to prevent conduction down the leads to the platinum coil.

The resistance measurements were made with a Griffiths resistance box (No. 7) especially designed for the measurement of platinum temperature. We are under great obligations to the University of Chicago for the use of this box, which was kindly loaned us for this work. The construction and calibration of a resistance box similar to the one used by us has been described by Mr. E. H. Griffiths in *Nature*, November 14th, 1895. The method there described was employed by us. Two independent calibrations of the coils and bridge wire gave practically identical results.

Temperature on the platinum scale is defined by the equation $pt = \frac{R - R_0}{R_1 - R_0} \times 100$, where pt denotes platinum temperature, R_0 the resistance of the thermometer at 0° C., R_1 the resistance at 100° C., and R the resistance at the temperature pt. Platinum temperature is therefore independent of the unit of resistance employed, and all of our measurements are given in terms of the mean box unit of Box No. 7 (approximately one mean box unit equals .01 Ohm). Griffiths gives as the values of R_1 and R_0 the following: $R_1 = 358.078$, $R_0 = 258.362$ (in mean box units, Box No. 6). From which the fundamental interval $R_1 - R_0 = 99.716$, and $\frac{R_1}{R_0} = 1.38596$. To reduce these values to true Board of Trade Ohms the factor for this Box (No. 6) at 20° C., as given by Griffiths, is 0.010003. We are uncertain as to the absolute value of the mean box unit of our Box (No. 7), and hence cannot yet compare our values in the standardization of our thermometer directly with those of Griffiths.

The following values of R_0 were obtained:—

258.471
258.472
258.469
258.471

258.471 mean box units, 20° C.

Each of these values is the mean of five or six measurements with different combinations of coils and bridge wire. It is interesting to note that the last two determinations suggest the possibility that the effect of radiation is appreciable in a room whose temperature is 20° above the thermometer coil; the first of these was made in a double wall vessel with ice in both compartments, the second immediately after in the same mixture of ice and water with the inner polished vessel removed.

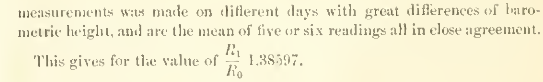
The determination of R_1 gave the following values:—

358.228
358.229
358.235

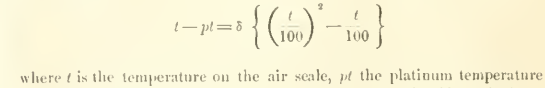
358.231 mean box units, 20° C.

These determinations were made in a hypsometer in which the thermometer was screened on all sides from radiation effects. Each of the above measurements was made on different days with great differences of barometric height, and are the mean of five or six readings all in close agreement.

6166.



6163.



This gives for the value of $\frac{R_1}{R_0}$ 1.38597.

The conversion from the platinum to the air scale is made by the following formula:—

$$t - pt = \delta \left\{ \left(\frac{t}{100} \right)^2 - \frac{t}{100} \right\}$$

where t is the temperature on the air scale, pt the platinum temperature as defined above and δ a constant. (For the experimental evidence in favor of this formula, see the elaborate comparison of the air and platinum thermometers by Callander and Griffiths, Phil. Trans Roy. Soc., Vol. 182 (1891), A). This relation being a parabolic one, only three temperatures are necessary for the complete standardization of a platinum thermometer, *i. e.* for the determination of δ. The identity of our value of $\frac{R_1}{R_0}$ with that obtained by Griffiths in his standardization of our thermometer, made it unnecessary for us to determine the resistance of the thermometer in boiling sulphur (the third point usually employed), and we therefore accepted the value of δ, 1.491, given by Griffiths, as correct.

All auxiliary thermometers used for resistance box, measurement of air temperatures, etc., were compared and reduced to air scale.

Each of the mercury thermometers was separately compared with the platinum thermometer in two independent series of observations.

The results of the comparisons are best shown by the accompanying curves, in which abscissae represent temperatures on Rowland's air scale and ordinates the corresponding corrections which must be added to Rowland's scale to reduce to the air scale obtained through the platinum thermometer.

The difference in the above comparisons are far greater than can be accounted for by experimental errors and must be partly due to the nature of the glass used in these thermometers, which seem more dependent upon their previous use than the hard glass thermometers. The depression of the zero in these thermometers is large. Thus in one experiment with 6163 the zero at the beginning, after the thermometer had been at about 20° C. for several weeks, was 60.11 mm.; after heating to 40° C. the zero was 59.77, a depression corresponding to .038° C.

A similar experiment with Baudin (No. 6165), which was exactly similar to these two thermometers, gave a depression of .021° C. after being raised to 30° C.

It will be observed that from 14° to 25° the range on the two air scales does not differ by more than a few thousandths of a degree. We must therefore conclude that the difference between the mechanical and electrical determinations of the mechanical equivalent of heat cannot be accounted for by differences in standards of thermometry, but must be sought in the energy determinations.

Changes Produced by Pressure in the Wave-Frequencies of the Lines of Emission Spectra of Elements.

By W. J. HUMPHREYS.

Several papers[1] have already appeared on this subject, and the present one, though containing certain new and interesting observations, is but a continuation of them and in no sense a complete account of the phenomenon of shift of spectral lines; nor could it be, as the investigation is still in progress.

As stated in the former papers, the shifts of the spectral lines were obtained by the use of an electric arc under pressure. The arc was formed in a closed cylinder provided with a quartz window; and the pressure was produced by pumping air into this cylinder to any extent desired—usually till the gauge registered from six to twelve atmospheres.

The particular concave Rowland grating used was of the largest class and one of the best ever made—the definition being exceptionally good; while the dividing engine on which the photographs were measured is practically a perfect piece of apparatus; the screw, though more than half a metre long, being from end to end without measurable error.

Several hundred photographs were taken and a large number of lines carefully measured. In fact the spectrum of almost every known metallic element has been examined at various pressures.

No attempt of course can be made, in an abstract like this, to give the details in regard to the apparatus used, the methods of research followed, or the results obtained. I shall therefore confine myself to a list, with brief remarks, of the relations observed between the shifts of spectral lines, the conditions under which the lines are produced and the elements producing them. I wish to state however that the labor of the investigation was spent in obtaining the facts in regard to the numerous lines examined, and not in searching after obscure and empirical relations; and I am quite ready to admit that some of the relations given may be more or less accidental or at least imperfect, while doubtless others quite as important have been overlooked. These relations are —

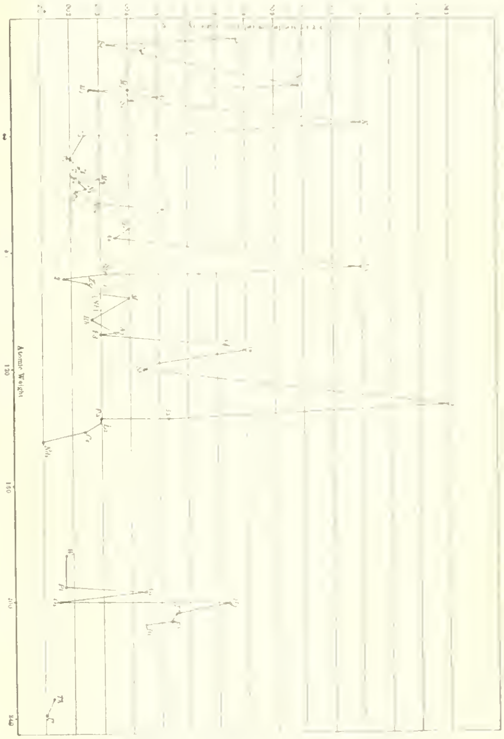
1. Increase of pressure causes all isolated lines to shift toward the red end of the spectrum.

This is entirely independent of the way in which the line spreads out, and is exactly the same for the line when reversed as when fine and sharp. Even those lines like the pair λ 3302 and λ [illegible] due to sodium which are shaded towards the side of shorter wave length give reversals that shift toward the red.

2. This shift is directly proportional to the increase of pressure.

3. It does not depend upon the partial pressure of the gas or vapor producing the lines, but upon the total pressure.

This statement is based upon a large number of experiments, but chiefly upon those that showed the shift of a given line to be constant at any pressure, no matter what the quantity of material used in the arc to produce it. The faintest trace of iron for instance, which occurred as an impurity in the carbon poles, gave lines that shifted neither more nor less than did the same lines when one of the poles was a solid iron rod.



4. The shift of the lines seems to be nearly or quite independent of temperature.

At any rate it is independent of the strength of the electric current (from two ampères to one hundred and eighty ampères) used to produce the arc; and recent work by Moissan[1] on titanium shows that the temperature of the arc probably increases with increase of current.

5. The lines of the "cyanogen" bands are not appreciably shifted.

This seems to be true of all the lines of the different so called "cyanogen" bands. Other bands, like those of aluminium oxide have not been examined.

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[1] Astrophysical Journal, February, 1896. Humphreys and Mohler.
Astrophysical Journal, October, 1896. J. F. Mohler.
Astrophysical Journal, November, 1896. W. J. Humphreys.

[1] Annales de Chemie et de Physique, October, [illegible]

6. The shifts of similar lines of a given element are proportional to the wave-lengths of the lines themselves.

7. Different series of lines (as described by Kayser and Runge) of a given element are shifted to different extents. When reduced to the same wave-length these shifts are to each other approximately as one to two to four for the principal, first and second subordinate series respectively.

8. Similar lines of an element, though not belonging to a recognized series are shifted equally (when reduced to the same wave length), but to a different extent than are those unlike them.

9. Shifts of similar lines of different substances are to each other, in most cases, inversely as the absolute temperatures of the melting points of the elements that produce them.

10. The shifts of similar lines of different elements are to each other approximately as the products of the co-efficients of linear expansion and cube roots of the atomic volumes of the respective elements (in the solid state) to which they are due.

11. Analogous or similar lines of elements belonging to the same half of a Mendelejeff group shift proportionately to the cube roots of their respective atomic weights.

That this may hold it is necessary to class sodium with potassium, lithium, rubidium, and cassium, which it resembles spectroscopically, rather than, as some tables give it, with copper, silver and gold, which spectroscopically it does not resemble. For similar reasons it is better to class magnesium with barium, strontium and calcium, rather than with zinc, cadmium and mercury.

12. The wave-lengths of those substances which, in the solid form, have the greatest co-efficients of linear expansion have the greatest shifts. The converse is also true.

13. The shift of similar lines is a periodic function of atomic weight, and consequently may be compared with any other property of the elements which itself is a periodic function of their atomic weights.

Of these relations 7 and 8, in their present forms, 12 and 13 have but recently been observed. It was impossible of course to obtain 13 till the spectra of a large number of substances had been examined. From 12 one might expect the unknown co-efficients of expansion of uranium, lanthinum, cerium, vanadium and others to be rather small, and those of lithium, rubidium, cassium, indium and a few others to be quite large.

The accompanying curve shows the periodic relation between the shifts of the lines and the atomic weights of their respective elements. The abscissae give atomic weights; and the ordinates, the shifts of the lines in thousandths of an Angström unit for wave-length 4000 produced by an increase of pressure of one atmosphere.

Surface Tension of Water and of Dilute Aqueous Solutions. By N. Ernest Dorsey.

During the past year I have been endeavoring to determine the surface tension of dilute aqueous solutions by means of the method of ripples. All work previously done on the surface tension of solutions has been on solutions of about one-half normal concentration, or greater, and most of the observers have deduced the surface tension from the measured rise of the solution in capillary tubes.

For at least two reasons the method of capillary tubes is open to serious objections. First, the height a liquid rises in a tube depends upon the angle between the wall of the tube and the surface of the liquid *where it meets the tube.* This contact angle can not possibly be measured, since the surface of the liquid lies entirely on one side of the point where we wish to know its inclination; and as we can measure the inclination of a finite surface only, every measured value of the contact angle must be too large.

The second objection is that probably the surface tension of the solution-glass surface, as well as that of the solution-air surface, varies with the concentration of the solution. If such is the case the surface tension found will depend upon two changes which can not be readily separated, and which render the interpretation of the results difficult.

For these reasons I decided to use the method of ripples, which was first successfully used by Lord Rayleigh, although with his arrangement of apparatus individual observations differ by about 2 per cent. After trying many plans one was finally adopted that gives individual results that agree to about $\frac{1}{3}$ per cent.; and the average departure of single observations from the mean of several seldom exceeds $\frac{1}{4}$ per cent.

The waves were generated by a fork whose frequency was often determined and was always near 62.87 double vibrations per second. The water and solutions were contained in a porcelain tray 1 by 12 by 14 inches. The wave length was measured by means of a telescope mounted on a dividing engine, whose screw had a pitch of 1.0328 mm. The waves were invisible under ordinary conditions, but were observed by Foucault's method for rendering visible small vibrations in plane or spherical surfaces.

The water used was especially distilled by Mr. W. T. Mather from chromic acid and alkaline potassium permanganate, and was condensed in a block tin condenser; it was the kind used by him for his electrolytic work. The salts were obtained from Eimer and Amend and were said to be chemically pure.

With this apparatus I have determined the surface tension of water and of solutions of sodium chloride, potassium chloride, sodium carbonate, potassium carbonate, and zinc sulphate, of concentrations varying from 0.05 normal to normal.

The value found for water is $T = 75.98$ dynes per centimeter at 0°C., while Sentis (Jour. de Phys. (3) 6, 183, 1897) working by an entirely different method found $T = 76.09$ at 0°C., which differs from the other by only 0.14 per cent. These values agree very well with the values given by Lord Rayleigh, Hall, Volkmann, and others, but are much lower than Quincke's value.

It was found that the surface tensions of dilute aqueous solutions are linear functions of the concentration; so that we may write $T_s = T_w + kC$, where T_s = surface tension of the solution, T_w = surface tension of water at the same temperature, k is a constant, C is the concentration in gram molecules per liter. Below is a table showing the values of k as determined by different observers.

		Dorsey.	Volkmann.	Quincke.	Rother.
NaCl	k =	1.53	1.59	1.57	1.38
KCL		2.23	1.41	1.57	1.47
$\frac{1}{2}$ Na_2CO_3		2.00	0.987	1.57	——
$\frac{1}{2}$ K_2CO_3		1.77	1.78	1.57	——
$ZnSO_4$		1.86	——	——	——

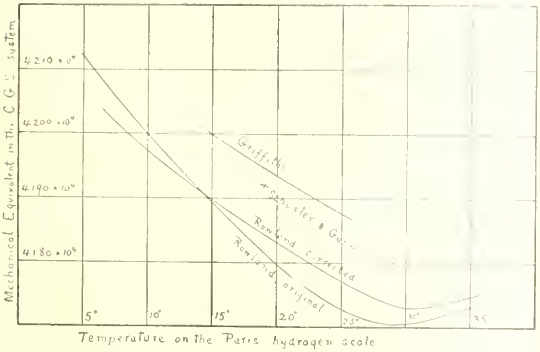
Volkmann found that the curve for Na_2CO_2 at great dilution becomes steeper than the one for K_2CO_3, which agrees with my results. Quincke's value, 1.57, does not agree with his results except for KCl and NaCl. I cannot account for the very high value I found for KCl, but it must be borne in mind that the values given above are not fairly comparable, since my values are for solutions generally less concentrated than $\frac{1}{2}$ normal, while the others are found for solutions of greater concentration.

A Re-calculation of Rowland's value of the Mechanical Equivalent of Heat, in terms of the Paris Hydrogen Thermometer. By W. S. Day.

The measurement of the mechanical equivalent of heat made by Rowland in 1877-79, (Proc. Am. Acad., 15, p. 75, 1879) is probably the best one in which the heat was produced by the expenditure of mechanical energy. Later determinations made with great care, in which the heat was produced by the expenditure of electrical energy, give results higher by about one part in four hundred. Rowland's measurement of temperature was based on comparisons made between an air thermometer and three Baudin mercurial thermometers, by which he reduced his measurements to the absolute thermodynamic scale. It was the object of the present investigation to compare his thermometers with the hydrogen scale of the International Bureau of Weights and Measures, at Sèvres, near Paris, and make a re-calculation of his value of the mechanical equivalent accordingly.

For this purpose, three Tonnelot thermometers which had been carefully studied at the International Bureau, and compared with their standards at several points of the scale, were obtained and compared with the three principal thermometers used by Rowland in his experiment. These comparisons were made in a horizontal comparison tank, designed and constructed for the purpose. Rowland's thermometers were originally compared with the air thermometer in a vertical position, and they were used in the same position in the calorimeter. This made it seem more natural to make the comparisons described here vertically also, but a number of reasons of a practical nature made the author decide to make them horizontally. The chief reason for this was the necessity of taking the zero point of the Tonnelot thermometers after each reading, this being the method used at the International Bureau, and which experience has shown to be the only accurate one. It was necessary therefore to apply a pressure correction to the readings on Rowland's thermometers, to reduce them to what they would have been if the comparison had been made vertically, but this was measured very accurately by an apparatus similar to that used at the International Bureau. In all other respects the attempt was made to use Rowland's thermometers in the way in which he used them in his experiments.

The Tonnelot thermometers had their zeros determined immediately after a measurement at any given temperature. The zero points were determined by plunging the thermometers into a vessel filled with finely crushed ice mixed with distilled water. The ice used was very pure artificial ice. The vessel in which the zero points were taken was constructed so that the thermometers could be immersed in the mixed ice and water to a distance of 7 cm. above the zero point. They were then read through a small ebonite tube that extended horizontally through the tank from one side to the other, and through which, at its middle point, the thermometer stem passed. By a simple device, the water in the tank was prevented from running out. The object of this arrangement was to make sure that all parts of the stem in which the mercury was, as well as the bulb, should be at a temperature of 0° C., and to avoid the presence of dew and of drops of water on the stem, which, in the usual way of taking zero points, often interfere with the proper reading. The thermometers were always read, in taking zeros, and in the comparison tank, by means of a micrometer, consisting of a reading telescope supplied with a micrometer screw that moved the whole telescope.



From the comparisons made, corrections were obtained for each of Rowland's thermometers, which, when applied to their indications reduced to the absolute scale by the tables given in his paper on the mechanical equivalent, would make them agree with the Paris hydrogen scale. From these corrections, Rowland's value of the mechanical equivalent was corrected, taking into account each individual experiment, the thermometers used in it, and the number of observations made with each thermometer. The original values, and the corrected values found in this way, are compared at several temperatures, in the following table.

Temp.	*Old.*	*Corr'd.*	*Griffiths.*	*Schuster & Gannon.*
6°	4209 × 10⁴	4204 × 10⁴		
10	4200	4197		
15	4189	4189	4199.7 × 10⁴	
20	4179	4183	4193.2	4191 × 10⁴
25	4173	4177	4187.4	
30	4171	4173		
35	4173	4174		

These numbers are in the C. G. S. system, on the hydrogen scale. The results given above are also represented by the curves drawn here. The corrected value of the mechanical equivalent found in this way is the same at 15° C., at 10° it is lower by one part in 1700, at 20° it is higher by one part in 1200, and at 25° higher by one part in 1000. The heat capacity of water as indicated by the corrected values of the mechanical equivalent varies at about the same rate as that given by the experiment of Griffiths. (Phil. Trans. 184 A. p. 361, 1893. Phil. Mag. 40, 437 and 447, 1895.) If therefore curves were drawn representing the specific heat of water between 15° and 25° as given by Rowland's experiment, and by Griffiths, taking the value found in each case at 15° as unity, the two curves would be practically identical. This seems to show that the difference between the value of the mechanical equivalent found by Rowland, and that found by Griffiths and by Schuster and Gannon using electrical methods may not be due to an error in the experiment itself, but to some error in the electrical standards of resistance or of electromotive force.

These results are in almost absolute agreement with those obtained by Mr. Waidner and Mr. Mallory. (See page 42.)

Velocity of Silver Ions in Aqueous and Alcoholic Solutions. By W. T. Mather.

The brilliant researches of Hittorf and the experiments made by others since have largely increased our knowledge concerning the velocities of the ions; nevertheless much still remains to be learned especially concerning the effects of changes in concentration, temperature and the solvent employed. The present paper contains the preliminary results of an investigation in these directions, the experiments being confined to solutions of silver nitrate in water and alcohol and silver acetate in water.

The apparatus designed for this purpose consisted, (Fig. 1) of two vertical limbs which were reduced in size at the upper ends and connected just at the top of the larger portions by a U tube having a stop-cock of large bore at its center. The knob-shaped electrodes, of pure silver, were secured in place and connected to the conducting wires by stems which passed through holes in the stoppers which closed the lower ends of the main limb. The exterior wires were insulated by being covered with thick walled rubber tubing. The whole was cemented into a light brass frame to permit of its being secured in a definite position to a fixed rod in the thermostatic bath or on the table. The reduced portions of the main limbs were graduated and the content of each half of the apparatus determined by calibration with mercury, the electrodes being protected by a thin coating of shellac. It contained about 95 cubic centimeters.

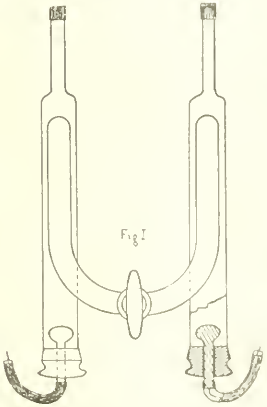


Fig I

For the investigation of the effects of temperature a new form of thermostatic bath was devised in which the liquid was protected from external influences by double walls. The space between the walls was filled with a mixture of glycerine and water which by its expansion, controlled a form of Bunsen lamp made for the purpose. Thermometers placed at different levels in the bath proved the temperature to be remarkably constant though no stirrer was employed. Experiments at 0° C. were made by placing the apparatus in a large tank containing water and finely powdered ice. The current was supplied by a battery of 10 storage cells and was measured by means of small silver voltameters, a calibrated Rowland D'Arsonval galvanometer being also used as a check upon the results and to determine the constancy of the current. The analyses were made by titration with ammonic sulpho-cyanate solution, ferric ammonium sulphate being used as an indicator. Especial care was taken in the preparation of all the solutions employed, the chemicals used being specially purified for this purpose.

In the preparation of the solutions in absolute alcohol and also in filling the apparatus with the same many precautions were necessary to prevent the absorption of moisture, since the alcohol, which had been dehydrated by means of copper sulphate, was extremely deliquescent. All the apparatus used was carefully dried and contact with the air of the room was prevented at all stages, siphons fixed in the bottles and flasks being employed for the removal of the solutions, all openings being closed with drying tubes filled with glass wool and dehydrated copper sulphate.

The method of procedure in an experiment was as follows: The apparatus was filled with a known volume of the solution and the upper ends of limbs closed with tubes having capillary openings. It was then clamped to a fixed rod in the path and the electrolysis allowed to proceed for several hours. The capillary openings were then closed with a little wax to prevent disturbance of the liquid and the apparatus removed from the path and clamped to the fixed rod upon the table. The level of the liquid was then adjusted and the central stop-cock turned. The contents were then washed out, filtered through washed asbestos to remove any particles of silver from the electrodes and finally analyzed.

The following tables give the results obtained:—t denotes the temperature of the bath; m the concentration of the solution in gram-equivalents per liter and v the relative velocity of the anion.

Silver Nitrate in Water.

t	m	v
0° C.	.1	.5414
0°	.1	.5407
0°	.1	.5413
29.1°	.1	.5317
47.9°	.1	.5272
47.4°	.1	.5286
47.7°	.1	.5280
0°	.025	.5377
45°	.025	.5246

Silver Acetate in Water.

t	m	v
0°	.025	.3746
0°	.025	.3727
28.3°	.025	.3818
45.4°	.025	.3890
44.7°	.025	.3896
49 4°	.025	.3873

Silver Nitrate in Absolute Alcohol.

t	m	v
19.5°	.10805	.5928
20.9°	.10805	.5926
20°	.10805	.5956

Silver Nitrate in 49.7 per cent. Alcohol.

t	m	v
24.4°	.1	.5316
23°	.1	.5329

Note on the Volhard Method for the Analysis of Silver. By W. T. Mather.

In the analysis of the results of the electrolysis in the above described experiments it was noticed that very different results were obtained according to the time taken in the analysis. Repeated experiments showed that if, after the first faint change of color was secured, the solution was allowed to stand even for a short time, the color grew gradually much darker, and if then a known amount of silver nitrate solution, of equal strength to the sulphocyanate, were added and the titration again completed, less than the same amount of the sulphocyanate solution was required to cause a return of the color. It was generally necessary to repeat this process several times before a constant state was reached. If, however, when this had been obtained, the solution was allowed to stand, the first tinge of color remained constant for several hours, finally fading out. The following results of one analysis will render this clear:—

AgNO N/10 added in c. c.	NHCNS N/10 required in c. c.	NHCNS N/10 corrected.
0.	94.89	94.89
.23	95.06	94.83
.51	95.30	94.79
.74	95.50	94.76
1.17	95.89	94.72
1.83	96.55	94.72
2.18	96.90	94.72

It was noticed that the precipitate first formed was very flocculent, even with the most rapid stirring, but gradually if the titration were repeated, as above, it became granular and settled quickly. The above results are probably due to occlusion of the sulphocyanate in the precipitate first formed. We are therefore forced to conclude that in analysis of silver compounds by this method, the titrations must be repeated several times in order to secure accuracy, it not being sufficient to take the first change of color as denoting neutralization.

In concentrated alcoholic solutions it was found impossible to secure good results, the nitric acid reacting upon the alcohol causing the formation of polymeric aldehydes, the brownish color of which prevented the determination of the point of color change of the indicator. Success was however secured when the alcoholic solution was diluted with water to about ten times its original volume before addition of the acid, the latter being added slowly with rapid stirring of the solution.

The Effects of Tension and Quality of the Metal upon the Changes in Length Produced in Iron Wires by Magnetization. By Byron Briggs Brackett.

All the specimens of iron wire experimented upon were about 2 m. long and 1.25 mm. in diameter. While under test a wire was suspended vertically so that tension could be applied by weights attached to its lower end. About the central portion of the wire was placed the magnetizing solenoid, 84 cm. long and with a free internal diameter of 3.6 cm., allowing no contact between it and the wire or parts of the apparatus attached to the wire. The variations in the length of 70 cm. of the wire within the solenoid was then observed by means of a mechanism, employing a lever and tilting mirror, in such a way as to magnify the actual changes about 25,000 times, and hence to give readings for $\frac{dl}{l}$ multiplied by 1,750,000.

For all tests made, with one exception, the changes in length caused by a definite magnetizing field were read both with the field on and after it had been removed. These readings were made for gradually increasing fields up to the limits of the apparatus, the points taken on each curve being very numerous and especially close together wherever the curve changes its direction rapidly.

All of these tests were followed by careful tests for the magnetic induction in the specimens under conditions identical with those of the elongation tests. From these, the induction in the wire is known for each elongation, and curves are plotted between elongation and induction as well as between elongation and magnetizing field.

Three different kinds of wire were tested: piano-wire in its natural condition: annealed piano-wire: and very soft annealed iron wire.

Results obtained.—

1. Natural Piano-Wire.

This wire could not be kept straight without considerable tension. For under the tensions employed (658 and 1949^{kg} per sq. cm.) no change in length was observed until the fields were reached at which the magnetization rises rapidly. Changes in length were first observed when the field was removed. This was an elongation and the length of the wire with the field off continued to increase up to the field strengths at which the induction curve turns towards the horizontal. From that point on, the wire was permanently elongated with the field off. But this value of $\frac{dl}{l}$ did not exceed 3.5×10^{-7} and could have been produced by the tension caused by adding about 10 grams to the weight already on the wire. At a very short distance beyond the point where the wire begins to lengthen as the current is broken, it shows its first change of length with the field on by beginning to contract. From this point on to the end, the wire contracts with the field on and the contraction increases apparently in direct proportion to the field until at the end, for a field of 300, $\frac{dl}{l}$ equals about 30×10^{-7}.

The curves for the different tensions differ but little.

2. Annealed Piano-wire.

After the piano-wires had been tested in their natural condition they were softened by annealing and the same wires were tested again. They then showed elongations with the fields both on and off up to the point where the induction curve turns towards the horizontal. The elongations here are greater with the field off than when it is on. But on each curve, the elongation increases nearly in direct proportion to the induction. Not far from the turning point of the induction curve, the elongations with the field off cease to show any change, this value of $\frac{dl}{l}$ not exceeding 7×10^{-7}. At about the same point the curve with field on begins to descend along a nearly straight line, just as it did before the annealing. But here the contraction is more rapid, the slope for the straight part of the curves, as plotted to field, being nearly 50 per cent. greater than before annealing.

Increased tension does not affect the general shape of the curve. But it diminishes the amount of the elongation and increases the final contraction with field on, while it causes both elongation and contraction to begin at weaker fields.

3. Soft Annealed Wire.

The curves for the very soft wire are in general exactly like those for the annealed piano-wire, except that the changes of length are very much greater, and in some cases for weak fields and slight tension, the wire shortens when the field is removed. But in these cases the residual induction in the wire is always much less than the induction with the field on. And finally the elongation with the field off becomes as great or even greater than the maximum elongation with the field on. After this the length tends to remain constant until the end of the experiment, although there is a very slight shortening for the strongest fields. This permanent lengthening for the soft iron gave a value of $\frac{dl}{l}$ as great as 33×10^{-7} for the least tensions used. The straight portion of the curves with field on shows a contraction that is much more rapid than for annealed piano-wire and its slope is more than twice as great as that for the natural piano wire.

Increased tension showed exactly the same effects as upon the annealed piano-wire, though the amount of the changes due to tension was much greater.

The complete series of experiments would tend to confirm Joule's statement that the changes in length caused by magnetization are greater in proportion to the [illegible] of the [illegible]. These [illegible] marked in the initial elongation than in the final [illegible].

The elongation curves [illegible] when [illegible] to [illegible] form and when plotted to induction [illegible] show [illegible] the magnetic induction in the wire [illegible] by the magnetizing field, directly proportional to the [illegible]. [illegible] induction the wire was [illegible] with the field off [illegible].

More than one application of the same magnetizing field gave [illegible] cumulative effect. Repeating any reading [illegible] the change [illegible] very little greater in the direction of the [illegible] but the [illegible] not significant.

The changes in length caused by adding and removing a [illegible] weight both before and during magnetization [illegible] Young's modulus due to magnetization. [illegible] of all the data of the complete series of experiments [illegible] Young's modulus with the intensity of magnetization [illegible] exceeding one half per cent, and nearly [illegible] the magnetization curve turns towards the horizontal. This [illegible] explains the reduced elongation and greater [illegible] with the larger tensions.

Preliminary Note on the Energy Spectrum of a Black Body. By C. E. Mendenhall and F. A. Saunders.

In the *Astrophysical Journal* for August, 1895, Dr H. F. Reid suggested the experimental study of this subject, based upon the theoretical deductions of Stewart and Kirchoff that the radiation existing [illegible] with walls at a uniform temperature would be independent of the character of the walls and would be that of an absolutely black body at that temperature, provided only that the walls had at least an infinitesimal absorptive power for all wave lengths. This work we have been carrying on, and though it is not completed, enough has been obtained to warrant a brief statement.

In order to study the radiation in the interior of a closed body it is of course necessary to cut a small slit in its walls and thus the theoretical conditions cannot be absolutely fulfilled; then again it is practically impossible to obtain an absolutely uniform heating of a body of considerable size. If it is found, however, that varying the size of the slit does not change the form of the curve then we can be reasonably sure that the first of these difficulties is an unimportant one. This point is now under investigation. As regards the non-uniformity of temperature it is reasonable to suppose that any curve will be a mean of the curves corresponding to several adjacent temperatures, and that it will not, in form, depart far from any one of them, if the departure from uniformity is not large.

The bodies we have used are cylinders of iron and of copper 6 inches high and 4½ inches inside diameter, and the ratio of the area of slit to the total inside area is [illegible]. One arm of the spectrometer reaches to within 3 inches of the furnace and carries the real optical slit which is [illegible] mm. wide. The radiation through this slit falls upon a rock-salt prism with faces 2 x 3 inches and then upon a lens of the same material, 3 inches in diameter, which focuses it upon the bolometer strips, which are of platinum foil, coated with platinum black, and these subtend an angle of five minutes at the axis of the spectrometer. The galvanometer is a four-coil one, has a resistance of three ohms, connected in parallel, and gives a sensibility of about 5.10^{-10} for 10 sec. complete period and a metre distance.

As far as our preliminary results show there is between 1000° and [illegible] Cent., a noticeable shift of the maximum of the energy spectrum towards the blue with rise of temperature. An absorption band, not due to water or to carbon dioxide, falls very near the maxima of our curves and often prevents these from being directly observed; this is probably due to gases filling the black body, entering from the gas furnace by the holes through which pass the platinum-platinum-iridium thermocouples used in measuring the temperature of the black body.

PROCEEDINGS OF THE SCIENTIFIC ASSOCIATION OF THE JOHNS HOPKINS UNIVERSITY.

One Hundred and Thirty-first Regular Meeting, February 18, 1897.

The President of the Association in the Chair.

The following communication was presented and read:

The so-called "Inheritance of Acquired Character." By WILLIAM K. BROOKS.

One Hundred and Thirty-second Regular Meeting, March 18, 1897.

The President of the Association in the Chair.

The following communications were presented and read:

1. The Projection of Panoramic Views of Contoured Surfaces. By JOSIAH PIERCE, JR.

An abstract of this paper was printed in *Science*, and a general statement of Mr. Pierce's work on this subject was printed in a recent number of the *Circulars*.

2. The Nerve Impulse in its Relations to the Strength of the External Stimulus. By C. W. GREENE.

Nerves may be partially or wholly isolated from the animal body and kept alive with comparative ease. This is true for both cold- and warm-blooded animals. When the sciatic of a frog, for example, is isolated and laid across non-polarizable electrodes in a moist chamber it will show the typical nerve-current of rest when the electrodes are put in connection with a sensitive galvanometer. And if the nerve be stimulated by electric stimuli, or otherwise, it will show the typical action current or negative deflection.

On the assumption that the action current is not only an accompanying phenomenon but a true index of the physiological activity of the nerve, Dr. A. D. Waller, of England, has made use of the phenomenon to investigate the question given in the title of this paper and to extend our knowledge of nerve physiology in other directions.

The Physiological Department of this University has recently acquired new and very sensitive instruments especially adapted to quantitative measurement of nerve currents, and for the measurement of interrupted currents of the strength used in physiological work. Both instruments are inventions of Dr. Rowland. With such advantages I have reinvestigated the relation between the strength of stimulus and the resulting action current.

In a given set of experiments the nerve was stimulated for twelve seconds once each minute until the desired range of strength of stimulating force was covered.

The plotted curve expressing the relation of the stimulus to the action current, stimuli plotted along the abscissa, resulting action currents along the ordinates, has the following character: First, beginning with the first effective stimulus and with very slight increased increments of stimulus, there is a rapid increase by equal increments of the action current giving an almost vertical limb of the curve. This extends to a strength above that necessary to produce a maximal muscular contraction. Second, there follows this first straight portion of the curve a still further increase, but by diminishing increments, producing a part strongly concave to the abscissa. This part of the curve is produced by the reaction to stimulating induced currents just perceptible to the lip or tongue. An increase of stimulus above this point, even to ten times the strength, produces only a slight rise in the curve, the whole region of supramaximal stimuli producing a curve expressed by a straight line slightly diverging from the abscissa.

This curve is given by nerves from the frog, the terrapin, and the dog. Frog nerves are the most irritable, and the reactions are most variable. Terrapin nerves are least irritable and give the smallest action current. The nerves of the dog give the most constant reactions and the vagus and the sciatic curves are essentially the same.

The following were presented and read by title:

1. A New Form of Mirror for Reflecting Telescopes. By CHARLES LANE POOR.

2. A New Form of Equatorial Mounting for Reflecting Telescopes. By CHARLES LANE POOR.

One Hundred and Thirty-third Regular Meeting, April 29, 1897.

The President of the Association in the Chair.

The following communications were presented and read:

1. On the Grouping of Neurones within the Central Nervous System with Especial Reference to the Localization of Nervous Functions. By LEWELLYS F. BARKER.

The application of the cell doctrine in its completeness to the study of the nervous system first became possible after the discovery of certain methods which could show us the nerve cell in its entirety. The nervous system is now known to be made up of a great mass of cells which while agreeing with other cells of the body in the possession of cytoplasm and nucleus and probably also of centrosome and attraction-sphere, differ much from them, particularly in their mode of branching and in the length of their processes. Each nerve cell, including all its processes, dendrites, axone, collaterals and telodendrions in accordance with the suggestion of Waldeyer, has been termed a *neurone*. In the nervous system there are altogether many hundreds of millions of these neurones and it is the task of modern neurology to study the forms of these structures, to follow their arrangement into groups and to make out the relations of the individual groups to one another. It has been shown that in at least the majority of instances each neurone is morphologically separate and distinct from every other neurone, being related to its fellows only by contact or contiguity, not *per continuitatem*. Further investigations have proven that the neurone is also the unit in physiological and pathological processes as might have been expected from its anatomical independence. In such a society of neurones as the nervous system represents, one would expect evidences of division of labor to be manifest, and that certain of the neurones should be set apart for certain functions, while others were reserved for other functions. Already much has been found out in this connection and we have the promise of further increase in our knowledge in the immediate future.

The methods which have proved of service in localizing groups of neurones and of ascertaining the functions of these groups, may be classified under several headings:

(1) *Strictly anatomical.* The study of the gross anatomy of the parts and of the coarser bundles by methods of dissociation, teasing, etc. (Meynert, Clarke); the minute analysis of the form relations as revealed by the gold-method (Gerlach); the silver chromate method (Golgi, Ramón y Cajal, v. Kölliker, v. Lenhossék, van Gehuchten, Retzius, Berkley); the vital staining with methylene blue (Ehrlich, Mayer, Dogiel, Huber); and the method of Nissl (Nissl, Hodge, Held, Lugaro, Marinesco, Flatau, v. Lenhossék).

(2) *Strictly physiological.* The study of the normal neural functions (including the psychic) manifested by man and animals.

(3) *Clinical-pathological.* In cases in which during life there have been disturbances or loss of neural and psychic functions, the nervous system has been studied after death with especial reference to the groups of neurones which have been affected (Türck, Weigert, von Monakow, Strümpell, Wernicke, Gowers, Osler, Mills, Dana, Starr, Charcot, Déjèrine and many others).

(4) *Experimental physiological.* Physiologists, by the use of electrical methods and other devices, have contributed much to knowledge by artificial stimulation of nerve endings, nerve fibres and nerve centres, and

at the same time watching the motor, secretory and other results (Du Bois-Reymond, Claude Bernard, Fritsch and Hitzig, Ferrier, Beevor and Horsley, and others).

(5) *Experimental pathological.* Injuring mechanically or chemically or extirpating groups of neurones or their processes and studying the results during life and after death (Flourens, Schrader, Goltz, Munk, Luciani, Marchi, v. Gudden, Nissl, Horsley, Mellus and others).

(6) *Comparative anatomical and physiological.* The study of the phylogenetic development of the nervous system (Edinger, Herrick, Meyer).

(7) *Embryological.* The ontogenetic investigation of the nervous system in man and higher animals supplemented by the study of function in young animals including child study (His, Flechsig, v. Bechterew, Preyer).

By the aid of these methods a vast mass of knowledge has been accumulated with regard to:

(1) The histology of the neurone as a unit and its functions as a unit.

(2) The anatomical grouping of neurones of the same sort and characterized by the same function (e. g. aggregations of lower motor neurones, peripheral sensory neurones and the like).

(3) The relations of groups of neurones of the same sort to one another (e. g. shorter and longer tracts associating the activities of the groups of lower motor neurones).

(4) The union of the activities of groups of neurones of different sorts with one another with formation of functional units of different orders (e. g. the sensory and motor mechanisms concerned with simple reflexes and complex reflexes).

(5) The reciprocal relations of combinations of groups of neurones of different sorts with one another (e. g. the mechanisms underlying the co-ordinated movements, instinctive reactions and the so-called voluntary movements). Some of the most striking examples coming within this category are the mechanisms of association inside the cerebrum itself, by means of which the primary sensori-motor areas in the cortex are indirectly connected with one another by means of areas in which are situated masses of association-neurones. The evidence in favor of differences in function corresponding to several great groups of association-neurones (Broadbent, Flechsig) was referred to.

(6) Lastly, the nervous system as a whole considered as an anatomical unit corresponding to a physiological unit which includes the whole neural and psychic activities of an individual or personality. Cases of double personality were referred to as well as the attempts which have been made to explain such phenomena.

The results of neurological study show a progressive increase in the complexity of nervous structures in the ascent of the animal series as well as in the development of a given individual. The main differences in individuals of the same species, as regards their nervous systems, are apparent not so much in the lowest groups of neurones as in the highest groups, in the most complex mechanisms. There are enormous differences in the structure of individual brains doubtless corresponding to the differences in manifestation of neural functions. No two of us have, for example, the same power of visual perception, the same store-house of visual memories, the same faculty of reproducing visual experiences or of associating them with experiences derived through the other senses. The speech area in the cortex, to cite another example, varies much with the capacities manifested by the individual during life (brain of Gambetta).

The mechanisms of the nervous system which are the last to appear are the least constant and most unstable. They are as a rule the most vulnerable, and the association centres of the brain have been well designated "the Achilles' Heel of the Nervous System" since they are as a rule the first to be seriously injured by certain poisons, notably alcohol and the toxines of syphilis. They are the ones chiefly involved when a man "dies from the top."

A man well endowed as regards his nervous system at birth, can have its constituent neurones deleteriously affected by injurious internal or external influences, while another, less generously provided for in the beginning, may still be capable of much if his environment be fortunate.

The conduct of an individual depends less upon the other organs of his body than upon the condition of his nervous system; broadly speaking, as his neurones are, so the man is. If this be true the practical, ethical and sociological deductions are obvious. The hope for the future lies (1) in the bringing into existence of individuals endowed with the best kind of nervous system, that is, in determining the most suitable admixture of ancestral germ-plasms and (2) in [illegible] of these individuals in the most [illegible] environments. The [illegible] can best be accomplished by [illegible] in two directions, [illegible] nutrition and the prevention [illegible] which will call forth the most [illegible] possible to the neurones with which the individual has started and [illegible] the protection [illegible] from external and internal [illegible] are limited.

2. A New Form of Reflecting Telescope. By Charles Lane Poor.

Dr. Poor explained the experiments that he has carried out in grinding and polishing a new form of parabolic mirror for reflecting telescopes. The mirror is a portion of a paraboloid of revolution cut at the extremity of the latus rectum. The reflected beam is at right angles to the incident light; no second mirror is therefore necessary; the full aperture of the mirror being used.

The advantages of such a form of mirror were pointed out and the great simplification in equatorial mounting indicated. The declination axis becomes the telescope tube, the image being formed at the intersection of the polar and declination axis and is always in the same position: the observer remains at rest while viewing any and every part of the visible heavens. No dome is required. Other advantages were indicated and several modifications of the general form pointed out.

The following papers were presented and read by title:

1. A Study of Nitrous Oxide Anaesthesia. By G. T. Kemp.

This research was undertaken to determine whether nitrous oxide mixed with oxygen will maintain anaesthesia of sufficient depth, for a sufficient time to warrant its use to replace ether and chloroform in major surgery. Also to determine whether nitrous oxide produces anaesthesia by asphyxia or by virtue of specific anaesthetizing properties of the gas itself.

Results: Animals, breathing nitrous oxide with sufficient oxygen to maintain life, become deeply anaesthetized and maintain this anaesthesia for a time sufficient to perform major surgical operations. If the nitrous oxide be replaced by nitrogen—the amount of oxygen remaining the same—the anaesthesia passes off. This shows conclusively that nitrous oxide has anaesthetizing properties not possessed by nitrogen.

Analysis of the gases of the blood during nitrous oxide anaesthesia shows that the blood carries sufficient oxygen to the tissues to maintain life. The carbon di-oxide is greatly reduced in quantity and there is strong evidence that general metabolism is lowered and that the system has the power to adapt itself to the changed conditions—including the lessened amount of oxygen in the inspired gas, as compared with air.

The safety of nitrous oxide in comparison with chloroform and ether renders it desirable that the gas should receive wider trial as an anaesthetic in major as well as in minor surgery.

A mixture of 90 to 95 per cent. nitrous oxide and 10 to 5 per cent. oxygen is recommended for the induction of prolonged anaesthesia.

2. Is the Active Substance in Suprarenal Extracts found normally in the blood of the Suprarenal Vein? By George P. Dreyer.

This question is really one of fundamental importance in the physiology of the suprarenal bodies. If the substance in question is simply a post-mortem product, as has been suggested, its study cannot throw much light upon the function of these organs.

Cybulski, in 1895, first attempted the answer by collecting the blood flowing from the gland and injecting it back into another vein of the same animal, the presumption being that if the gland normally secreted this substance the blood thus collected would set up effects at least similar in kind, though perhaps less in degree, to those produced by the extract of the gland.

From a single experiment of this kind, Cybulski concluded that it was present in the blood and this result was confirmed [illegible] who again, however, recorded an isolated experiment. As the evidence so far was meagre and as, moreover, Oliver and Schäfer in 1895 were not able to reach the same result, it seemed desirable to accumulate evidence by means of a more extended series of experiments. The results were surprisingly regular and constant and decidedly confirmed the work of Cybulski.

While the blood from an ordinary systemic vein produces either no change in blood-pressure and pulse-rate, or only slight irregularities, adrenal blood gives changes like those following injections of the extract.

3. The Regeneration of the Posterior Roots of the Spinal Nerves. By Messrs. Baer, Dawson and Marshall.

While regeneration of peripheral nerve fibres is an assured fact, it has remained unsettled whether intracentral nerve fibres are also capable of regeneration. These experiments were intended to determine this point, and the results seem to show quite positively that regeneration is possible within the central nervous system. The experiments were made by ligating the posterior root of the second spinal nerve under aseptic precautions, and allowing an interval of several weeks or months for regeneration to take place. At the end of this time the peripheral branches of the second spinal nerve were stimulated and the effects upon blood-pressure and respirations were recorded. It was found that distinct reactions were obtained upon both the vaso-motor and the respiratory centres, thus proving that the posterior root fibres had regenerated in the cord and made normal connections with the centres named.

4. The Physiological Effects of Injections of Extracts of the Hypophysis Cerebri. By W. H. Howell.

The experiments were made with glycerine and saline extracts of both the glandular and the infundibular lobes of the hypophysis. These extracts were injected into the circulation of normal animals. It was found that while the extracts of the glandular lobe had little noticeable effect, those of the infundibular lobe caused a pronounced slowing of the heart rate and an increase in blood-pressure. The former of these effects persists in part after section of the vagi, indicating that it is due only partially to an action on the nerve centres. The rise of blood-pressure is caused mainly by a peripheral constriction of the blood vessels. As compared with suprarenal extracts the effects obtained with these extracts were characterized by their longer duration and by the fact that their inhibitory action on the heart is not entirely removed by giving atropin or cutting the vagi.

5. The Relation of Arterial Tension to the Volume of Blood Circulating Through the Brain. By W. H. Howell.

In a number of papers upon the mechanics of the circulation in the brain it has been assumed that a dilatation of the arteries beyond a certain limit must result in a compression of the cerebral veins and a diminution of the volume of the blood-flow, owing to the fact that the expansion of the arterial walls is transmitted through the brain enclosed in the rigid skull as an increased intra-cranial pressure which causes a compression of that part of the vascular system where the pressure is least, namely, the cerebral veins. Experiments upon living animals in which the arterial expansion was produced by increasing the tension in the arteries supplying the brain, have shown that within the limits that can be reached by physiological means every increase in general arterial tension is followed by a greater flow through the brain. The author has attempted to complete these experiments by using an artificial circulation and raising the pressure in the arteries supplying the brain to as much as 500 mms. of mercury. The venous outflow from the brain was recorded graphically, and it was found that the outflow increased proportionally to the pressure in the arteries. There was never any indication of even a temporary diminution in the flow through the brain. The details and method of the experiments will be published subsequently.

NOTES IN PHILOLOGY.

Custom and Myth in the Midsummer Night's Dream.

By Henry Wood.

[Abstract of a Paper read before the University Philological Association, October 16, 1896.]

In the Eddic Skirnismál, as in the Midsummer Night's Dream, the betrothed lovers are confronted with a *trinoctium* of waiting, before the 'night of their solemnities.' On receiving the message from Gerðr, the god Frey exclaims:

> Long is one night! two are still longer!
> How can I endure to wait three?
> A month has often seemed shorter to me
> Than this half wedding-night (hý-nótt).

Shakespeare begins the Midsummer Night's Dream with Theseus' speech:

> Now, fair Hippolyta, our nuptial hour
> Draws on apace; four happy days bring in
> Another moon: but, O, methinks, how slow
> This old moon wanes!

The word *hý-nótt*, to which Niedner (Z f d A, 30, 137 seqq.) and Axel Kock (Ib. 40, 198) have given other meanings, cannot be sundered from its natural congeners, O. H. G. *hîwo*, O. N. *hjón* 'man and wife,' Germ. *Heirat*, etc., without violence. No sufficient reason has been given for departing from Vigfusson's rendering 'wedding nights.' Frey's messenger, Skirnir, must be supposed to have spent one of the three nights on his return journey; Frey could therefore speak of this 'half wedding-night' (cf. Engl. sennight, fortnight) as still remaining.

In the Midsummer Night's Dream, Theseus and Hippolyta mention 'four nights.' These are now shown to represent three, reckoned inclusively. There is a similar disparity in the Skirnismál between the nine nights of waiting imposed by Gerðr and the three nights of Frey's impatient soliloquy. But the contradiction is only apparent, and may be reconciled. In her epic Lay, in the Edda, Brunhild directs that the sword, as *symbolum castitatis*, be placed between her body and that of Sigurd, on their funeral pile. 'Lay it again, just as when we two lay in one bed, and were called by the names of man and wife.' Later on, when riding to the underworld, she is met by an ogress who reproaches her for her deeds in life. Brunhild answers by recounting her relation to Sigurd: 'We slept and lay in one bed, as if he had been my brother. Neither of us laid a hand on the other for *eight* nights.' But in the Volsunga Saga we are told that Sigurd, when he wooed Brunhild for Gunnar, stayed *three* nights, in Gunnar's likeness, 'and they lay in one bed together; but he took the sword Gram and laid it betwixt them: then she asked him why he laid it there, and he answered that in that way he must needs wed his wife or get his bane.' It is evident that the nine and the three nights are susceptible of explanation as two parallel conceptions of the period of abstinence exacted by custom.

The Skirnismál and the Midsummer Night's Dream may be called respectively the earliest and the latest Germanic drama on Summer's advent. The occurrence of the *trinoctium* in such surroundings, on a theme in common to both, cannot be accidental. As a *nox conjugalis expectatrix*, it intervenes in both monuments between formal betrothal and marriage. But this is, historically speaking, merely a variant of an earlier prescribed interval immediately after marriage. The ancient custom is found in India, in the Biblical Apocrypha, and in Greece.

The Gṛhya-Sûtras, the canons of domestic life, directed that each newly wedded pair should pass the three nights succeeding their marriage, lying upon the ground and tending their marriage fire, and should so long refrain from consummating their nuptials. The most interesting chapters of the Ragnar Lodbrok Saga are based upon the same rite, and supply several details which further illustrate the Midsummer Night's Dream. In Mediaeval Christian Europe the custom became an ecclesiastical requirement. This could be waived, if the privilege of conjugal union immediately after marriage was purchased as an indulgence. Dr. Karl Schmidt, Jus Primae Noctis, Freiburg, 1881, has collected material, but his book [illegible] being exhaustive. Among further sources, now for the first time [illegible] in this connection, are Wolfram von Eschenbach's Parzival and the [illegible] of Huon of Bordeaux. The analogy between the latter story and the Midsummer Night's Dream is very close. Oberon appears in both [illegible] Providence of the lovers, but also as their judge. He is the [illegible] guardian of marriage. In this function he officiates in the last scene of the play, and dispenses blessings for a full compliance with nuptial ceremonies.

This analogy between both stories amounts to a complete correspondence with the church custom above mentioned. The priest sprinkled the bed with holy water, 'wodurch die Teufel, Hexen und Schrattle (Alp) verhindert wurden, den Eheleuten schaden zu können.' Birlinger, Volksthümliches aus Schwaben, ii, 401. The 'field-dew consecrate,' which Oberon makes use of for blessing the bride-bed, is fairies' holy water. In the Middle Ages this ceremony marked the church's permission that the bridal couple should begin married life. But both ecclesiastical law and custom prescribed in many places that the blessing should be pronounced, not on the actual day of marriage, but three nights afterward.

In the Skirnismál the same penalties impend over Gerðr, which actually overtake Titania in the Midsummer Night's Dream. Oberon corresponds to Frey, and Gerðr to Titania. The plot of both productions has to do with the repentance of an erring lady-love. Titania's punishment for scorning conjugal duties, and the threat against Gerðr's maiden [illegible]ness are identical. Gerðr represents in general terms the Spring, which must be wooed and won each year by the god. She answers therefore to both conceptions, that of maid and of wife. The triple curse of 'lewdness, love-madness and lust,' pronounced upon her, is precisely what Titania is visited with. The three-headed monster lover, whose threatened embraces Gerðr hastens to shun, is the 'true-begotten father' of Bottom with his ass's head.

In the Ragnar Lodbrok Saga the royal pair was punished with deformed offspring, because the rite of the trinoctium was violated. In the Midsummer Night's Dream Oberon promises to 'all the couples three' immunity in their issue from all 'the blots of Nature's hand,' from 'marks prodigious, such as are despised in nativity.' The reason for this immunity is now seen to lie in the observance of the three wedding nights, which form the basis of the whole plot.

The second part of the paper is devoted to a discussion of this body of tradition, in the Elizabethan period in England. It is shown that Shakespeare's play marks the close of a long development of folk-lore conceptions, which include Proserpina, Queen Mab and Titania. In tracing the ascertainable steps of this development, particular attention is paid to the Pervigilium Veneris, the Roman Lemuria, the May Marriage, and the lyrics of Thomas Campion.

In the third part of the paper a new time-scheme for the play is constructed. According to this scheme, which demands the explanation of accompanying text in detail, the action must be supposed to begin on the twenty-eighth of April, and to end on the first of May at midnight.

Immemorial Germanic tradition had peopled these nights just preceding the first of May, with wayward spirits and their doings. Goethe's Faust preserved their mad revelries, in the 'Walpurgisnacht,' but Shakespeare's unerring hand has depicted the hierarchy of spirits in their original character and function. Shakespeare's Oberon remains the mysterious king of shadows, whose own redemption from the ills that beset him engages our fleeting sympathies, only in so far as befits beings and visions 'no more yielding but a dream.' But as guardian of marriage he enters [illegible] and beneficently into the human life around him. He has even a personal stake in the happy outcome of the plot. These human loves and their righteous fulfillment are the pledge and seal of peace in Oberon's household of nature, but late convulsed by domestic infidelities and [illegible] capable of righting itself. It is not enough that he, as marriage's vicegerent, should exact full compliance with 'sanctimonious ceremonies,' [illegible] 'with full and holy rite.' In the excess of his solicitude Oberon [illegible] dons cape and cowl and officiates as lay priest in the last scene, [illegible] the 'couples three' and their issue. The story turns on the [illegible] he affords to marriage rites, justly performed, and the penalty he is ready to exact for the non-observance of the period of abstinence.

The Midsummer Night's Dream has hitherto always seemed rather the fairies' play than ours. All the characters had something eldritch about them. The [illegible] they had found in the play [illegible] fairies' loves in which [illegible]

But the truer outline [illegible] elves themselves, though [illegible] with foreign [illegible] ness and [illegible] performance [illegible] thoroughly human [illegible] of fairy, but [illegible] represented as binding [illegible]

> Wo die [illegible] K[illegible]
> Ergreifen alle Welt[illegible]

The dream [illegible] of the play [illegible] has not the 'margin of [illegible] half the [illegible] of Shakespeare's [illegible] editors who, [illegible] scenes into a series of wild [illegible] toward their readers very much [illegible] lote in her advice to Charlotte [illegible] the Midsummer Night's Dream [illegible] selves sane purposes and a regulated [illegible] and look for them in the author of this play.

Goethe's Sonnets. By Henry Wood.

(Abstract of a paper read [illegible] 1896.)

Goethe's Sonnets are meeting with the [illegible] Shakespeare's. The question: "who was their [illegible]?" threatens to [illegible] every other consideration. While critics apportion them to Minna Herzlieb, Bettine Brentano, Princess Caroline of Weimar, etc., the interest of the literary public at large has languished.

Recent attempts in this vein merely renew old [illegible] of fair claimants for dower rights in the sonnets. [illegible] new material, or to consider the larger questions [illegible] Herman Grimm, *Goethe und die W[illegible]* [illegible] still unapproached. It seems more necessary than ever to emphasize the fact that Goethe is the hero of his own sonnets. [illegible] from that point of view to estimate their true [illegible] among his works, and as parts of the larger Goethe-Autobiography.

The similarity between the first two sonnets and [illegible] poem *Mahomets Gesang* is well known. A certain correspondence in the [illegible] has been noted in AfdA. xiv, 137. But it is not [illegible] similar tropes. The connection between the two poems, [illegible] restless current of the poet's life, is organic. The first part of the present study points out and applies these correspondences. Nos. VI and VII, Traveller's Fare and Leavetaking, have afforded Kuno Fischer [illegible] sea voyage which he makes the lover undertake. But this [illegible] the sonnets a second *Alexis und Dora*, exists only in Fischer's imagination. From the poet's voyage there is no return.

The second quatrain of No. I, in its original form, [illegible] the word 'dämonisch.' The real significance of its later [illegible] in the sonnet is explained by a literary reminiscence.

> Quorum ope, cum volui, ripis mirantibus amnes
> In fontes rediere suos. Ovid, Metamorphoses, VII, 199.

The effect of the Oread's [illegible] plunge [illegible] in Goethe's sonnet by: 'Die Welle [illegible] zurück,' which answers to the [illegible] of the Latin poet. 'Zurückstaunen' [illegible] figurative variation of the [illegible] 'to [illegible] up,' and only the [illegible] reading of the MS. [illegible] the word from emendation. But it is now evident that [illegible] was [illegible] by Goethe to express the [illegible] 'ripis mirantibus amnes.' It is Ovidian.

Ovid's heroine is represented as [illegible] whose [illegible] have a specific end in view. The renewing [illegible] is expressly associated with the magic power of making a stream ascend to its source. To this is then added:

> Vivaque saxa sua convulsaque robora terra
> Et silvas moveo.

The power to restore youth and cause water to flow uphill was thus suggested to Goethe in intimate connection with a third faculty, that of dislodging rocks and uprooting forests, and sweeping both away by magic-charmed streams. The poet of *Mahomet's Gesang*, seeking an embodiment for his 'Sonettenwut,' seized upon this material and wrought it into our finished poem of modern magic, which might be styled 'Goethe's Metamorphosis.'

The fundamental conception of the sonnets, that of the poet's life, arrested but again flowing resistlessly onward, did not permit him to make any extended use for himself of the result of Medea's charms. This idea had already been worked out in Faust, the First Part of which was to be published in the following year (1808). Faust's new life really dates from the 'Einschläferungslied' or Lullaby. The Witch's Kitchen, which contains his official rejuvenation, merely affixes a coarse dramaturgical stamp to this scene, and seems intended mainly for the pit. If the Witch's Kitchen contains witchcraft, this contains magic, old as Ovid and as full of symbolism as the sonnet. Precisely the same image is used in both cases. The lake in the sonnet, and the dam which holds it, answer to the wine-streams broadening out into lakes,

> Ums Genügen
> Grünender Hügel.

The word 'Genügen' in this passage has been explained as what confers satisfaction (was solches Genügen gibt); cf. Hildebrand in Grimm's Wörterbuch. But this rendering is quite unsupported by clearly-marked examples. Nothing in the history of the word can justify such a wrenching from the usual meaning 'satisfaction, contentment.' It is subjective, not objective. The wine-brooks broaden out to lakes around hills, not for the satisfaction which the hills in their greenery confer upon the beholders, but for the contentment of the hills themselves. This statement is now also susceptible of proof, from a comparison with the first sonnet.

In the second quatrain the Oread plunges into the waves, 'Behagen dort zu finden.' The mountain and forest, which she symbolizes, are set in motion by the whirl of her descent, and fall in a landslide into the stream, damming it up to a lake. This dam or hill finds in the cool waters the 'Behagen' that the Oread sought. In the last verse it is the 'glitter of the wave plash against the rock,' which is called 'ein neues Leben.' The poet is the now eager element, the wave, and his mistress, the hill and rock, is wooed by his waters. The two words 'Behagen' and 'Genügen' express the same idea, in Faust and the sonnet. 'Inniges Behagen,' 'inniges Genügen' are synonymous terms. K. E. Ebert (Dichtungen, 1828) writes:

> 'Und fühle ein innig behaglich Genügen.'

The 'Einschläferungslied' and the sonnet represent one conception, worked out with varying effect but with the same apparatus. Faust has some thirty years taken off his back, and hastens to explore 'the sensual depths; to quench the fervors of glowing passion.' Goethe, the poet, now fifty-eight years old, becomes enamored of a miss of seventeen. But here the resemblance ceases. Goethe, many years later, told Zelter that he had loved Minna Herzlieb 'mehr als billig war,' but even in this sinister conjunction with the Faust tragedy she remains a blameless episode. The real heroine of the sonnets, for which Minna was the lay figure, is a poetical creation of which Ottilie in the Elective Affinities is the most familiar example.

The fate of both pairs of lovers in Goethe's novel is intimately associated with a lake. Before the entrance of the Captain and Ottilie into the family circle, this lake had been merely a chain of three ponds with a low outlet. But no sooner did Edward fall in love with Ottilie, than he directed that the work of damming up the ponds and stream into a lake should be begun. The first catastrophe follows, during the celebration and display of fireworks at the lake, on Ottilie's birthday. The dam gives way under its load of spectators; part of it is precipitated into the lake, and a young life is nearly sacrificed. Charlotte, Edward's wife, feels it to be a warning and breaks up the celebration, but Edward persists in his course: 'No, Ottilie!' he calls out; 'the extraordinary occurrence of this evening brings us more quickly together. Thou art mine!' Thus he blindly pulls on his fate. A whole series of correspondences, all connected with the lake, follows this. When grouped together for examination, they are seen to be quite sufficient in number and quality to justify the claim that the central idea of both sonnet and novel is one and the same.

The assertion that the 'Wahlverwandschaften' would rank equally high as a work of art, without the symbolism of the sonnet, would be in a certain sense true. But it would leave out of account the fact that Goethe wrote his works for the world and for himself. This tribute to personality which genius reserves the right to pay in ciphered confessions is inseparable in its origin from the motive power that produces great works. In the present instance, the evidence from the Elective Affinities is valuable, as furnishing additional proof that the sonnets are no mere botanical herbarium, containing the pressed specimens of our poet's 'Johanoistrieb' of love, but rather a deep-thoughted 'Metamorphose der Pflanze,' a chapter in the evolution of his greatest works. Ottilie, the St. Ottilie of Goethe's Strassburg days, represents the type of resignation for Goethe's earlier and later years. Her figure now stands out also in bold relief in her niche in his sonnet chapel.

From the point of view of sonnet criticism, it may not be too much to hope that apologists for Goethe's sonnets will soon lack an audience. Adopted as the sonnet form was by him for the briefest period only and even then under protest, he has filled it with graceful and pensive thoughts, such as cluster around a memorial urn. Unequal in parts, the sonnets all without exception express something specifically Goethean. They are his pleasures, his woes, his fate, which here find characteristic utterance. The brief love episode, the pain at parting, the conscious relation to Petrarch, are genuine, but all these concern the poet of 1807 merely. Through them and in them another Goethe speaks, not with 'new-born sighs and denizened wit,' but as the author of Faust and the Wahlverwandschaften, secure in his inviolate personality, should speak,

> 'When to the sessions of sweet silent thought
> I summon up remembrance of things past.'

PROCEEDINGS OF UNIVERSITY SOCIETIES.

Scientific Association.

February 18, 1897. One hundred and thirty-first regular meeting.
March 18. One hundred and thirty-second regular meeting.
April 29. One hundred and thirty-third regular meeting.
(*See report of Proceedings, pages* 48–50 *of this Circular.*

Philological Association.

May 21, 1897. One hundred and fifty-ninth regular meeting. Professor Gildersleeve in the chair. Twenty-six members were present.

Papers read:

On the Development of the Alexandrine Verse, by H. P. Thieme.
On the Usage of Quotiens and Quotienscumque in Different Periods of Latin, by O. F. Long.

Historical and Political Science Association.

April 23.

Relations of Geography to History, by G. C. Lee.
Hobson on the "Problem of the Unemployed," by C. H. Brough.
A Virginia Community, by L. N. Whealton.
Historical Bibliography, by George Iles.

May 7.

Quit-Rents and Currency in North Carolina, by E. W. Kennedy.
Freytag's Luther, by L. F. Schmeckebier.
Fairbanks' Sociology, by P. L. Kaye.
Bourinot's Canada, by W. T. Thom.

May 21.

Introduction to Southern Economic History, by J. C. Ballagh.
An Early Maryland Land Grant, by B. B. James.
Andrews' Historical Development of Modern Europe, by J. R. Ewing.
Rabbeno's American Commercial Policy, by F. R. Rutter.

Young Men's Christian Association.

Tuesday, April 13.—Missionary address by Rev. Dr. W. H. Ballantyne.
Tuesday, May 4.—Election of Mr. M. L. Barr as General Secretary for the ensuing year.

JAMES JOSEPH SYLVESTER.

MINUTE OF A MEMORIAL MEETING.

BALTIMORE, MAY 2, 1897.

A meeting of the Faculty of the Johns Hopkins University, with other friends of the late Professor Sylvester, was held in McCoy Hall, May 2, 1897, to commemorate the life and services of this eminent mathematician. The chair was taken by the President of the University, Dr. Gilman, who made a brief introductory address. He was followed by Dr. Fabian Franklin, lately Professor of Mathematics in this University, who delivered the Commemorative Address given below. Afterwards, Professor Gildersleeve repeated and enlarged upon the estimate of Professor Sylvester's services, adopted by his colleagues when he left Baltimore in 1884. Sir Archibald Geikie, F.R.S., of London, then gave some reminiscences of the last years of Sylvester's life. The closing addresses were made by Professor Remsen and Professor Newcomb. By a formal assent the company reaffirmed its former expressions of admiration and gratitude, in a minute prepared by Professor Gildersleeve.

President Gilman's Introductory Remarks.

We are here to bring together our tributes of respect, admiration, gratitude, and affection called forth by the death of that illustrious mathematician, whose presence inspired the faculty and students of the Johns Hopkins University during the first seven years of its history.

Sylvester, in any seat of learning,—in Cambridge, where he was an undergraduate; in Oxford, where he held the Savilian professorship of geometry; in London, where he felt at home during all the intervals of professional duty; in Paris, where he was a frequent and welcome visitor; in Baltimore, where he passed one of the most prolific periods of his fruitful career; in the University of Virginia, where he touched and glanced; in Woolwich, where he taught the cadets,—in any and every seat of learning, indeed in every company of intellectual men, Sylvester was pre-eminent. A man of genius, like some other men of genius, he had his eccentricities. These were the necessary, the uncontrollable, manifestations of his remarkable psychological nature, and they cannot be forgotten,—but they must not conceal from us the brilliant imagination, the powerful will, the splendid enthusiasms for literature and science, the capacity for abstract thought, the never failing ingenuity and fertility, and the marvellous aptitude for mathematical discovery that distinguished his career.

Among us he has left enduring memorials. His manifold suggestions, by some of which we are still governed; his relations with Cayley, Kelvin, Rayleigh, Huxley, Spencer, and other eminent Englishmen who came to Baltimore during his residence among us; his establishment of the American Journal of Mathematics, now beginning its twentieth volume; and above all the impulse that he gave to mathematical science in this country and the influence he exerted upon his pupils, many of whom are now distinguished as teachers and investigators,—all these considerations (and more that might be named) entitle him to lasting and honorable remembrance in this University, apart from the manifold considerations which entitle him to a place of renown among the great promoters of mathematical science during the nineteenth century.

Address Commemorative of
PROFESSOR SYLVESTER

By Fabian Franklin, Ph. D.,

Of Baltimore, lately Professor of Mathematics in the Johns Hopkins University.

We have come together to do honor to the memory of the great man whose work in initiating and for seven years conducting the mathematical department of this institution, will always remain one of the proudest traditions of the Johns Hopkins University. To me, as one who was long his pupil, and who owes so much to his inspiration, has been assigned the task of saying something about the work and the genius of Sylvester, and especially about the influence which he exerted, while in Baltimore, upon the study of mathematics here and upon the advancement of mathematical research in America.

Since his death, there has appeared in the English journal, *Nature*, and has been reprinted in the *Johns Hopkins University Circulars*, a review of his life and work by Major MacMahon; and in 1889, when that work was well nigh ended, Sylvester's great compeer and friend, Professor Cayley, contributed to the columns of the same journal a sketch of Sylvester's labors. One of his Baltimore pupils, too, Professor Halsted, of the University of Texas, has given, in the columns of *Science*, an account of his life and achievements. It is therefore the less necessary to undertake here to give anything in the nature of an enumeration of even his most important contributions to mathematics.

His influence upon the development of mathematical science rests chiefly, of course, upon his work in the Theory of Invariants. Apart from Sir William Rowan Hamilton's invention and development of Quaternions, this theory is the one great contribution made by British thought to the progress of pure mathematics in the present century, or indeed since the days of the contemporaries of Newton. From about the middle of the eighteenth century until near the middle of the nineteenth, English mathematics was in a condition of something like torpor. The second half of the eighteenth century was one of the most brilliant periods in the history of mathematics; but the magnificent achievements of Euler, Lagrange, Laplace, awakened no response on the other side of the narrow seas. It seems almost incredible that the complacent conservatism of Cambridge went so far that even the notation of mathematical analysis as used on the Continent was untaught there until about 1820. Babbage tells us, in his "Passages from the Life of a Philosopher," how he, together with Herschel, Peacock, and a few others, founded in 1812 the "Analytical Society" for promoting (as Babbage humorously expressed it) "the principles of D-ism in opposition to the Dot-age of the University." It is from the translation by these three men (in 1816) of Lacroix's Treatise on the Differential and Integral Calculus, together with the publication by them, four years later, of two volumes of illustrative examples, that the first impulse toward a revival of mathematics in England is usually dated. Nothing could show more thoroughly the insular and retrograde condition of English mathematics in the early part of this century. The sticking to Newton's fluxions and dots, and the barring out of Leibnitz's differentials and d's, may be set down as a consequence of the great Newton-Leibnitz controversy; but, whatever the cause, so complete a separation from the great current of European thought implies stagnation deep-seated and not easily to be removed. And, accordingly, it proved to be the case that in the magnificent extension of the bounds of mathematics which was effected by the Continental mathematicians during the first four decades of the present century, England had no share. It is almost literally correct to say that the history of mathematics for about a hundred years might be written without serious defect with English mathematics left entirely out of account.

That the like statement cannot be made in regard to the past fifty years is due pre-eminently to the genius and labors of three men: Hamilton, Cayley, and Sylvester. Hamilton was a high and solitary genius, who constructed and developed, unaided, a great mathematical method. Great as was this work, it lay so entirely apart from the general line of research that it did not, in his own time at least, awaken widespread activity on the part of others either at home or abroad. On the other hand, the Theory of Invariants had a history of what may be called the normal type. Its origin is to be found in Boole's discoveries of isolated instances of invariance; these led Cayley to institute a systematic investigation of this remarkable and significant phenomenon; and Cayley's researches awakened the ardent interest of Sylvester. Under the hands of these two great masters, a new and important province was rapidly added to the domain of algebra. Not only did other English mathematicians join in the work, but Hermite in France, Aronhold and Clebsch in Germany, Brioschi in Italy, and other Continental mathematicians seized upon the new ideas, and the theory of invariants was for three decades one of the leading objects of mathematical research throughout Europe. It is impossible to apportion between Cayley and Sylvester the honor of the series of brilliant discoveries which marked the early years of the theory of invariants. Their names are linked together as the creators of a new and beautiful development of algebra, the ideas of which have profoundly influenced the progress also of geometry and of analysis generally. "The theory of invariants," says MacMahon, "sprang into existence under the strong hand of Cayley, but that it emerged finally a complete work of art, for the admiration of future generations of mathematicians, was largely owing to the flashes of inspiration with which Sylvester's intellect illuminated it." It is pleasant to know that the triumphs of neither were marred by any dispute as to personal claims or by anything even approaching jealousy. On the contrary, these two men of genius, antipodes of each other in temperament and habits of work, were alike in the constancy of their mutual friendship, regard, and admiration.

I have dwelt thus long on Sylvester's connection with the creation of the Theory of Invariants, because it is by that chiefly that he left his trace upon the history of mathematics in its large outlines. But his genius is quite as strikingly shown in researches of a more isolated character. Ten years before the date of his work in invariants, he wrote in quick succession several remarkable memoirs on algebraic subjects, especially on Sturm's functions and on elimination. His researches in the Theory of Partitions of Numbers are among the most original and remarkable of his works. In the Theory of Numbers he was especially interested in Ternary Cubic Forms. The question of the distribution of prime numbers had a great fascination for him; and he succeeded, while in Baltimore, in making an impression upon this recondite problem in that he contracted the limits found by Tchebycheff for the number of primes contained within a given range. His work seldom touched on geometry, but his "theory of residuation" in connection with cubic curves is a beautiful structure, to which he made some remarkable additions while in Baltimore. I am not, however, attempting to give a survey of his work; suffice it to add that, in addition to the subjects named, he made contributions to astronomy, to dynamics, and to the theory of link-motion, besides other special subjects.

One of the most striking of Sylvester's achievements was his demonstration and extension of Newton's unproved rule concerning the number of the imaginary roots of an algebraic equation. Newton had left no trace of the process of thought by which he had arrived at his rule, nor had he given any indication of the basis on which it rests. All attempts of later mathematicians to establish it had proved futile. It was characteristic of Sylvester to set himself the task of filling up this lacuna in mathematics. The things that attracted and fascinated him were of two kinds, which may be called opposite to each other. On the one hand, he revelled in any new and prolific method; the feeling of creation, of abounding productiveness, was to him as the breath of his nostrils. It was largely this that made the Theory of Invariants so congenial to him. To see a whole new world, full of unexpected and harmonious relations, expanding before him, was to fill him with an absorbing and exuberant enthusiasm. In the case of invariants, it may be said that his joy in this sense of creation was not even confined to the discovery of theorems; the algebraic forms themselves were to him as living beings, and the processes, invented largely by himself, for causing these creatures of the mathematical intellect to generate their kind, were to him a source of genuine delight.

Alongside of this love of prolific creation, another intellectual bent, on the surface at least of quite the opposite character, was equally strongly marked in Sylvester. Any crucial problem, especially one that was associated with the name of one of the great masters, if once it attracted Sylvester's attention, fastened itself upon his mind with a grip that seemed never to slacken its tenacity. It kept coming up, again and again, for years, and, as long as it remained unsolved, seemed to become periodically a source of unrest and discomfort to his mind. He had not the serenity which belonged to many other great mathematicians, and notably to Cayley, and which in a great measure permitted them to choose among the possible subjects of thought such as they deemed most profitable to pursue. With Sylvester, such tranquil and deliberate choice was entirely out of the question. His temperament was essentially poetic, and it would have been as impossible for him to concentrate the powers of his mind on one subject when the current of his thought was setting toward another, as it would have been for Burns to decide in cold blood to write a poem like Highland Mary or The Daisy when the inspiration of Tam O'Shanter was upon him.

It was the mention of Sylvester's demonstration of Newton's rule that suggested these reflections. We, who knew him well in later years, can find no difficulty in understanding the hold this problem had upon him. It was the good fortune of his early hearers in this University to be present when he came into the lecture-room, flushed with the achievement of a somewhat similar task. A certain fundamental theorem in the Theory of Invariants, which had formed the basis of an important section of Cayley's work, had never been completely demonstrated. The lack of this demonstration had always been, to Sylvester's mind, a most serious blemish in the structure. He had, however, he told us, years ago given up the attempt to find the proof as hopeless. But, upon coming fresh to the subject in connection with his Baltimore lectures, he again grappled with the problem, and, by a fortunate inspiration, succeeding in solving it. It was with a thrill of sympathetic pleasure that his young hearers thus found themselves in some measure associated with an intellectual feat, by which had been overcome a difficulty that had successfully resisted assault for a quarter of a century. Nor was this the only instance in which we had an opportunity of observing the tenacious hold upon his intellect of any problem that had come to assume in his mind the aspect of a challenge to the powers of mathematicians.

I have said that Sylvester's powers were set in motion by two opposite kinds of stimulus; that of abundantly rewarding results, and that of the stubborn resistance of concentrated difficulty. In both these kinds of endeavor, he achieved many and signal triumphs. That intermediate kind of effort which slowly and patiently builds up and improves and perfects one's own work, and which gives minute and prolonged study to the work of others, he did not command in any notable degree. He seemed incapable of reading mathematics in a purely receptive way. Apparently a subject either fired in his brain a train of active and restless thought, or it could not retain his attention at all. To a man of such a temperament, it would have been peculiarly helpful to live in an atmosphere in which his human associations would have supplied the stimulus which he could not find in mere reading. The great modern work in the theory of functions and in allied disciplines, he never became acquainted with. No one who witnessed the flaming up of his energies when, at the age of 62, in Baltimore, he felt himself for the first time among a band of enthusiastic young workers pursuing pure mathematics for its own sake, can doubt what the effect would have been if, in the prime of his powers, he had been surrounded by the influences which prevail in Berlin or in Göttingen. It may be confidently taken for granted that he would have done splendid work in those domains of analysis, which have furnished the laurels of the great mathematicians of Germany and France in the second half of the present century.

Cambridge, his natural intellectual home, would have been far less helpful, since it was examinations and not research which gave tone to the mathematical life there. But Cambridge would, of course, have been immeasurably better than the situations in which he actually found himself for forty years after his winning of the Second Wranglership. From a career at Cambridge, to the great loss of that University, of himself, and of mathematics, he was debarred by the religious tests then obtaining in the old English Universities. Professor Halsted, in his account of Sylvester's work already referred to, points out how the vicissitudes of his career were reflected in the richness or the meagreness of his mathematical production from period to period.

The life and work of Sylvester illustrate, in a striking way, the futility of the dispute as to the relative importance of native qualities and of external circumstances in determining the achievements of great men. If any man was ever an original genius, with consuming ardor for one intellectual pursuit, with love and devotion to it burning in youth and undiminished in age, Sylvester was such a man. If any province of thought is open to every worker in it, to work in as he pleases, uninfluenced by the doings of those who happen to be in his neighborhood, in his university, in his country, one would say that mathematics is that province. Yet no one could know Sylvester without feeling that, great and original as was his genius, environment must, in his case, exercise an extraordinary influence on its activity. He was sensitive, passionate, fiery; the glowing language in which he habitually indulged in the midst of his mathematical memoirs, was but a reflection of his ardent and excitable temper. Such a man must needs be keenly subject to depression and exaltation, to fits of apathy and ardor, according to the nature of his surroundings and experiences. Those who knew him cannot fail to be convinced that, eminent as were his actual achievements, they do not afford a true measure of his mathematical powers, in comparison with those of his great contemporaries. For he was at once less advantageously circumstanced than they, and in an exceptional degree subject to the influence of his surroundings.

Of his work as a teacher, I can speak only upon the basis of his activity in this University. The one thing which constantly marked his lectures was enthusiastic love of the thing he was doing. He had in the fullest possible degree, to use the French phrase, the defect of this quality; for as he almost always spoke with enthusiastic ardor, so it was almost never possible for him to speak on matters incapable of evoking this ardor. In other words, the substance of his lectures had to consist largely of his own work, and, as a rule, of work hot from the forge. The consequence was that a continuous and systematic presentation of any extensive body of doctrine already completed was not to be expected from him. Any unexpected difficulty, any suggested extension, such as would have been passed by with a mention by other lecturers, became with him inevitably the occasion of a digression which was sure to consume many weeks, if indeed it did not take him away from the original object permanently. Nearly all of the important memoirs which he published while in Baltimore arose in this way. We who attended his lectures may be said to have seen these memoirs in the making. He would give us on the Friday the outcome of his grapplings with the enemy since the Tuesday lecture. Rarely can it have fallen to the lot of any class to follow so completely the workings of the mind of the master.

Not only were we thus privileged to see "the very pulse of the machine," to learn the spring and motive of the successive steps that led to his results but we were set aglow by the delight and admiration which, with perfect naïveté and with that luxuriance of language peculiar to him, Sylvester lavished upon these results. That in this enthusiastic admiration he sometimes lacked the sense of proportion cannot be denied. A result announced at one lecture and hailed with loud acclaim as a marvel of beauty, was by no means sure of not being found before the next lecture to have been erroneous; but the Esther that supplanted this Vashti was quite certain to be found still more supremely beautiful. The fundamental thing, however, was not this occasional extravagance, but the deep and abiding feeling for truth and beauty which underlay it. No young man of generous mind could stand before that superb gray head and hear those expositions of high and dear-bought truths, testifying to a passionate devotion undimmed by years or by arduous labor, without carrying away that which ever after must give to the pursuit of truth a new and deeper significance in his mind.

As is well known, Sylvester had an extraordinary faculty for the coinage of words, which, indeed, was merely a part of his remarkably keen sense for language in general. In this matter of the coinage of words, he doubtless went to extremes, as he did in other things; but there can be no question of the great service he rendered to the new science of invariants by the creation of a whole vocabulary which rendered possible the crystallization of thought in what would otherwise have been a comparatively amorphous mass. There are doubtless other departments of mathematics which would be made more manageable by the skilful application of just such a name-creating faculty. Any mathematical conception with which Sylvester had much to do had to be equipped with a name. He justly felt that the absence of it impeded thought, and he could not be comfortable in this state of things. His hearers will not forget how, after getting along for some time with the notation $\phi(n)$, by which mathematicians had been content, from the time of Legendre, to designate the number of numbers less than a given number and prime to it, he came into the lecture-room one afternoon, and began, in his most emphatic manner, thus: "Gentlemen, I am about to introduce to you a name that has been struggling for birth for a century!" I may mention here an instance of his delicate sense for words—and indeed for things—which occurred during a walk I was taking with him. We were speaking of Mitchell, then a fellow in mathematics here, and I said that he impressed me as having a resemblance to Abraham Lincoln. He seemed struck with the idea, and, after a moment's silence, said, "Yes, there is a certain not inelegant stiffness about him which reminds one of Lincoln." Where Sylvester got his impression of Lincoln I do not know; but surely it would have been difficult to hit off the outward effect of the man in words more accurately chosen.

Another direction which his talent for expression and his love of the niceties of language took was that of versification. He made some excellent translations from Horace and from German poets, besides writing a number of pieces of original verse. The *tours de force* in the way of rhyming, which he performed while in Baltimore, were designed to illustrate the theories of versification of which he gives indications in his little book called "The Laws of Verse." The reading of the Rosalind poem at the Peabody Institute was the occasion of an amusing exhibition of absence of mind. The poem consisted of no less than 400 lines, all rhyming with the name Rosalind (the long and the short sound of i both being allowed). The audience quite filled the hall, and expected to find much interest or amusement in listening to this unique experiment in verse. But Professor Sylvester had found it necessary to write a large number of explanatory footnotes, and he announced that in order not to interrupt the poem he would read the footnotes in a body, first. Nearly every footnote suggested some additional extempore remark, and the reader was so interested in each one that he was not in the least aware of the flight of time, or of the amusement of the audience. When he had dispatched the last of the notes, he looked up at the clock, and was horrified to find that he had kept the audience an hour and a half before beginning to read the poem they had come to hear. The consternation on his face was greeted by a burst of good humored laughter from the audience; and then, after begging all his hearers to feel at perfect liberty to leave if they had engagements, he read the Rosalind poem.

Sylvester was quick tempered and impatient, but generous, charitable and tender-hearted. He was always extremely appreciative of the work of others, and gave the warmest recognition to any talent or ability displayed by his pupils. He was quick to fly into a passion on slight provocation, but did not harbor resentment, and was always glad to forget the cause of quarrel at the earliest opportunity. I know from extremely good authority that, in his intercourse with Professor Cayley, toward whom he maintained a lifelong and devoted friendship, and the admiration of whom might be said to amount to reverence, little episodes of this kind were not absent. Some fancied injury would lead Sylvester to write Cayley an angry letter; Cayley, who was as even-tempered as Sylvester was passionate and excitable, would quietly leave the letter unanswered. In a few days another letter was sure to come from Sylvester, written as though nothing whatever had happened. The mention of Cayley leads me to recall an incident of the farewell reception given to Cayley in Hopkins Hall at the close of his residence here, which affords another illustration of Sylvester's felicity of expression. The platform was abundantly decorated with flowers, and Cayley, who was extremely shy and retiring, looked very uncomfortable in his conspicuous position upon it while Sylvester was speaking. Referring to Cayley's modesty, Sylvester suddenly turned toward him and said, "There he sits, like a victim decked with flowers!"

Sylvester did not, I believe, like to speak about religion. He was born a Jew, and was buried in the Jewish cemetery at Dalston. I suppose that he would not have subscribed to any formulated creed; but he was a man of truly reverent mind, and a sincere theist. It was notable that in speaking of designs for the future, he quite habitually used the phrase, "please God," with an accent that showed it was not a mere form of words. Once, when I asked him what was his estimate of Clifford, he said, with great earnestness: "Clifford is a very great genius; I only wish he would stick to mathematics instead of talking atheism."

Of Sylvester's influence upon this University, not only through his teaching, through the foundation of the American Journal of Mathematics, and through the constant stimulation of mathematical interest here by his incessant productiveness, but also through the infection of his enthusiasm which was felt in every department of the University, it would be impossible to speak too strongly. His aggressive and singular personality seemed to act the part of a ferment which spread itself through the entire body of the University. In its prosperity and progress and fame he took the deepest interest, and his attachment to it was not weakened when he returned to his native land at the call of the University of Oxford.

Professor Sylvester's residence at the Johns Hopkins University constitutes an episode quite unique in the history of mathematics and of education. Up to the time when he came to America, the study of the higher pure mathematics may be said, with almost literal truth, to have been non-existent in our country. He came, a man who had almost filled out what is usually spoken of as the allotted span of life, and at once inspired zeal and activity in a field which had been almost uncultivated among us. The earliest outward effect of his labors was the foundation of the American Journal of Mathematics, the first mathematical journal of any importance ever published in America, and almost the first journal devoted to any scientific specialty. It may truly be looked upon as the father of that army of scientific journals which have since overspread the country and testified to the growth of the higher learning among us. The prestige of his name and the fertility of his work could not do otherwise than excite emulation in other American centres of learning. While there doubtless would, in any case, have been progress in this direction, it must be set down as preeminently the result of Sylvester's presence in Baltimore that mathematical science in America has received the remarkable impetus which the last twenty years have shown. American names are no longer absent from the record of mathematical progress. We have not yet produced one of the heroes of mathematics; but there are now among us a dozen universities in each of which something, be it much or little, is being added to that splendid monument of human thought which bears the record of conquests made by so many of the intellectual giants of the race.

Among these giants, Sylvester has without question the right to be reckoned. In the history of mathematics, his place will not be with the very greatest; but his work, brilliant and memorable as it was, affords no true measure of his intellectual greatness. Those who came within the sphere of his personality could not but feel that, through the force of circumstances combined with the peculiarities of his poetic temperament, his performance, splendid as it was, had not adequately reflected his magnificent powers. Those of us who were connected with him cherish his memory as that of a sympathetic friend and a generous critic. And in this University, as long as it shall exist, he will be remembered as the man whose genius illuminated its early years, and whose devotion and ardor furnished the most inspiring of all the elements which went to make those years so memorable and so fruitful.

PROFESSOR SYLVESTER AT OXFORD.

[Reprinted from *The Oxford Magazine*, May 5, 1897.]

Our last number had hardly appeared, when Oxford heard with sorrow of the death in London of the great old man whom for comparatively few years it had claimed for its own as Savilian Professor of Geometry. He passed away on Monday, March 15, and on the following Friday was followed to the grave with affectingly simple ceremonial in the Jewish cemetery at Ball's Pond.

The work of his older years only has been Oxford's. Cambridge, where he was Second Wrangler in 1837, boasts of him as her alumnus: but Dublin can in a real sense pride herself on being his alma mater. For those were the days of tests. Not only could his Cambridge College (St. John's) give him no fellowship, but the University could give no degree to one of the Hebrew persuasion. Dublin was ahead of the two other ancient Universities in the relaxation of tests; and to Dublin accordingly he turned for such academic recognition as a degree affords. It is needless to say that Cambridge did all it could to honour him when it could, and that he died an honorary fellow of St. John's.

The incidents of his life, the posts he held, the honours which were bestowed on him by bodies representative of learning and science all the world over, cannot be here dwelt upon. Even the work he did must be lightly passed over. His mathematical writings, though not so numerous as those of the renowned *confrère* Cayley, with whom more than with any other he will be for ever associated, amount in number to some hundreds, and are widely distributed through British, American and foreign journals and volumes of Transactions. Among them we will only mention first the grand series of memoirs, of which, and of Cayley's simultaneous researches, the whole theory of Invariants is the lasting result; secondly, the masterly development in his Oxford period of the Differential Invariants to which he gave the name Reciprocants; and thirdly, his brilliant investigations in connexion with the Partitions of Numbers, given some forty years ago in outline only by himself, but now at length promising to receive the attention which they deserve.

He was a veritable seer of Mathematics. A matter under immediate consideration he dealt with, not, as others would, as an object of interest in itself, or in connexion with what had preceded, but as something with endless ramifications stretching into the unknown in all directions. His prophetic eye and his impatient pen passed on with a rapidity which at once stimulated and dazed. None like he could see a subject in all its bearings, none could so point others towards fields in which to exercise their own more modest powers.

As a lecturing professor in Oxford he suffered from much unwarranted despondency—unwarranted, for his stimulating influence was ever at work, even when he felt least confidence in the fact. His historic course of lectures on Reciprocants in 1885-6 roused into productive activity which might well give him just pride an attentive class, composed mainly of graduates, and his courses on mere everyday subjects, strong as they were with individuality, were full of suggestiveness which did wonders for those among his hearers whose aims were not limited by a narrow interpretation of requirements for the Schools. But despondent over his lecturing work he undoubtedly was, and the feeling of discouragement grew upon him. It may be that, finding himself as an old man among a well-organized band of younger teachers, who were working on a sound though unpretentious system in which they had themselves been trained, he felt with craving that the time of life was gone when he could have moulded the ordinary routine of teaching, and worked with others merely as *primus inter pares*. It may be also that, not being primarily a geometer in the narrower sense of the word, he vexed himself with a precise interpretation of the letter of the duties of the occupant of the Savilian chair as imposed by the latest statute, namely to lecture on Pure and Analytical Geometry, forgetful that the new professorship to which the other and, to him, more congenial part of his predecessor's duties had been assigned, was then not actual but a mere contemplated possibility. Whatever the cause, this lowness of spirits aggravated the ailments of increasing age, and in particular made failing eyesight more than he could bear and continue to work. Accordingly in 1894 he applied for leave to resign the active duties of his chair, and a permanent deputy professor was appointed. Since then he has lived for the most part in London, and has found little strength for mathematical activity. But towards the end his old fire returned, and the fatal seizure came upon him pen in hand.

Perhaps the most valuable direct outcome of his residence among us was the foundation of the Oxford Mathematical Society, a still vigorous institution of which he was originator and first president. The members of that Society, even more perhaps than the attendants at his formal lectures, have been impressed and excited to emulation as they have seen his always commanding face grow handsome with enthusiasm, and his eyes flash out irresistible fascination, while he expounded his latest discovery or brilliant anticipation.

He has done much for us here, but his Oxford work was after all but a fitting supplement to a long life's achievements. His fame is world-wide and enduring. Not only was he one of the few who half a century ago led the revival of Mathematics in England, but he almost alone was later the inspiring guide of the youthful ardour of a now brilliant school of Mathematics in the United States of America.

He was a man of wide culture and of poetical imagination, picturesque and even florid in his literary style. Known terms never sufficed for the expression of his ideas. New words appeared in every page he wrote, new sometimes as necessary for the expression of entirely new technical conceptions, new or newly used often as suggested by some apt but unexpected parallel in other realms of thought. His greatest recreation was to turn to poetical composition—indeed, this he at times pursued with a zeal akin to that devoted to his life's real work. His mastery of the technique of versification, on which he was an author, was always a pride to him.

Sensitive to a painful degree, the good opinion of others and just appreciation from them were at all times necessary for his happiness. Placed suddenly, when far on in life, among strangers, men widely different from and mostly generations younger than himself, he was not above suffering keenly at times from loneliness. But all the same there was sympathy in him, and more, for younger men's hopes and aims. We have been helped by his friendship, as well as by his genius; and we knew too that he valued us as friends, even though new ones when the old would have been better.

SYLVESTER'S "SPRING'S DÉBUT."

[Reprinted from *The Academy*, London, May 27, 1897.]

In addition to the translations given in his quaint and little-known *Laws of Verse*, which has long been out of print and is hardly likely to be printed again, the late Prof. Sylvester was the author, when at the Johns Hopkins University, of a rhyming *tour de force* called *Spring's Début*, privately issued in 1880. In itself the effort is interesting for the astounding variety of knowledge postulated by its abstruse rhymes, but there also are interesting notes. *Spring's Début*, which is a fantastic poem in praise of one Mary Winn, of Baltimore, consists of two hundred and thirteen seven-syllabled lines, all ending in rhymes or assonances to this lady's name. Here, for example, is a passage of parallels to the accomplished lady:

"Fine as point-lace or Mechlin,
Circe—but more feminine,
Pamela, artless genuine,
Like loved child of Cymbelin'—
Sainted, sweet-souled Imogen,
Not more lively, dear Nell Gwynne,
Dreamier, nocturne of Chopin,
Swan-song of weird Lohengrin,
Calmer, aureoled Capuchin
Niched beneath stone baldaquin,
Loved of the Dryads Evelyn,
Daintier touch of Maturin,
Purer, Rill of tears, Undine,
Shedde to undoe eache staine of sinne,
Prouder self-ruled, Catherine
Ere rose might of Mazarin
Or who broke to rein Berlin
High Autocrator Tzarin,
Pious as Evangeline."

It will be seen that explanatory notes are likely to be needed before the verses are done. As they proceed they become more and more far-fetched.

"He has never consulted, or even had in possession or held in his hand, a rhyming dictionary," says the author proudly, "which it is preposterous," he adds, "to suppose could be of any use in the invention of rhymes running over centuries of (let no ungentle reader be tempted to interpolate 'damnable') iteration." The reader, one feels, must be rather disposed to. Prof. Sylvester, it seems, began his feat after hearing the lady whom he praises and a friend of his own exchanging badinage as they passed down Charles-street; whereupon he set to work "intoxicated with the bright sun shining overhead, the balmy air, the song of the birds and the new come out Virgin Spring just beginning to peep over Old Father Winter's revered shoulder." History and geography are ransacked to supply the rhymes. Ledru-Rollin, Santorin, Guatemozin, Noor-ed Din, Custrin, Szegedin Debreczin, Lin, Apraxin, Golovin, Hamelin, Trissotin—these are a few of the words, all introduced relevantly and with pertinent epithets, which, had the author omitted notes, might puzzle the reader. An examination paper in *Spring's Début* would floor most candidates.

The notes are entertaining. In explaining who de Cormenin was Prof. Sylvester wrote, "By the causticity of his pen he sealed against himself the door of the Academy;" adding slyly, "Not more indispensable is snuff to a Highlander than incense to the assembly of the Immortals." In another place he justifies a clipped final "g" in "wainscotin'" by telling the reader to see "The Courtin'" by the accomplished United States Minister at the Court of St. James *passim*." That was, of course, the late Mr. Lowell. Finally, Prof. Sylvester puts on record a very neat pun:

"The author of this expression," he says, "having been called upon to acknowledge a toast to the Universities of the Old World [at a students' reunion in Baltimore], excused himself for being unprepared with a set speech on the ground that he had been busy all the morning, and on the previous evening, when about to arrange the order of his ideas to meet the emergency, had received an invitation (too agreeable to decline) to join some ladies in their box at the Opera . . . to which Mr. Teackle Wallis responded from the chair that he accepted with pleasure the explanation of his honourable friend who had just sat down, and hoped that he would always continue to act in accordance with the maxim 'Opera non verba.'"

A sketch of Professor Sylvester, written by Professor Cayley, may be found in the series of "Scientific Worthies," published in *Nature*, London (see *Nature*, January 3, 1889). An engraved portrait of Sylvester appears in the same number of the journal. Separate copies of the engraving can be had from the publishers.

The last number of these *Circulars* contains a biographical sketch by Major P. A. MacMahon, Professor in the Royal Artillery College, Woolwich, England, a friend of Professor Sylvester's, and a conspicuous contributor to the same field of mathematical science.

In *Science*, New York, April 11, 1897, can be found an article on Sylvester by Professor George Bruce Halsted, now of the University of Texas, one of Professor Sylvester's earliest pupils in Baltimore.

In the *Academy*, *Athenæum*, *Times* and other London papers, March 16-22, 1897, appeared biographical sketches and tributes.

SIR ARCHIBALD GEIKIE'S VISIT TO AMERICA.

[Reprinted from [illegible]]

The geologists of America have had the opportunity, during the last few weeks, of greeting Sir Archibald Geikie, F.R.S., D.C.L., Director-General of the Geological Surveys of Great Britain and Ireland, who has visited this country upon the invitation of the trustees of the Johns Hopkins University to give a course of lectures at that institution. These lectures were provided for by the generosity of Mrs. George Huntington Williams, which thus commemorates her husband, late professor of inorganic geology in that university who died in 1894. The arrival of Sir Archibald Geikie in New York was marked by a reception which was extended to him on Monday, April 19th, by the New York Academy of Sciences, in the American Museum of Natural History. Addresses of welcome were delivered by Professors Stevenson, Kemp, and Heilprin, to which the distinguished visitor responded.

On Wednesday, the 21st, Sir Archibald Geikie began his course of lectures at the Johns Hopkins University upon "*The Founders of Geology*," which embraced a treatment of the rise of geological thought from the days of the cosmogonists down to the opening of the present era. The influence of the older geologists in the development of modern geology was presented in an entirely new light, as the result of the exhaustive researches of the lecturer. Although many attempts at writing the history of geology have been hitherto made, the true significance of many of the earlier views and their first incorporation in the geology of the close of the past and the opening of the present century, had never been fully told. These lectures, which will appear in book form next fall, will add a volume of much importance to the geological treatises of the present day.

Invitations were sent out by the Johns Hopkins University to the geologists throughout the country, more than fifty of whom were in attendance upon all or a portion of the course. Excursions were arranged in honor of Sir Archibald, under the auspices of the Maryland Geological Survey, by Professor William B. Clark, the state geologist. The shorter excursions took place almost daily, during the season of the lectures, into the region adjacent to Baltimore, including a trip, as guests of the Secretary of the Navy, to the U. S. Naval Academy at Annapolis, where the Cretaceous and Tertiary geology of the Severn river was examined from a government vessel.

After the close of the lectures a longer excursion, occupying four days, was undertaken by the entire party to examine the geological section presented by the state of Maryland. The state board of public works granted the use of the state vessel, "Governor McLane," while the presidents of the several railroads in the state offered free transportation to the participating geologists. Upon the first day of the excursion, Wednesday, April 28th, an extended trip was made down the Chesapeake bay to examine the Cretaceous and Tertiary formations exposed along the [illegible]. Returning to Baltimore the party entered their special cars for the western section of the state, where the following day was spent as the guests of the president of the Consolidated Coal Company in examining the coal fields of the Alleghenies. The same night the governor of Maryland tendered a reception to the visitors at his home in Cumberland.

Upon the following day the Silurian and Devonian formations of the central Appalachian districts were examined, including a stop at Harper's Ferry, W. Va. The last day of the excursion, Saturday, May 1st, was spent in the Blue Ridge mountains in examining the Cambrian formations and the pre-Cambrian volcanic rocks, the trip closing with a visit to the Gettysburg battle-field. Those who took part in the excursions numbered nearly sixty and were as follows:

F. D. Adams, McGill College, Montreal; Cleveland Abbe, Jr., of Washington; R. M. Bagg, Johns Hopkins University; W. S. Bayley, Colby University; F. Bascom, Bryn Mawr College; George F. Becker, U. S. Geological Survey; L. A. Bauer, University of Cincinnati; J. M. Clarke, Assistant State Geologist of New York; Samuel Calvin, State Geologist of Iowa; W. B. Clark, State Geologist of Maryland; N. H. Darton, U. S. Geological Survey; J. S. Diller, U. S. Geological Survey; C. W. Dorsey, U. S. Department of Agriculture; S. F. Emmons, U. S. Geological Survey; O. L. Fassig, U. S. Weather Bureau; Sir Archibald Geikie; D. C. Gilman, Johns Hopkins University; L. C. Glenn, North Carolina; R. T. Hill, U. S.

Geological Survey; J. A. Holmes, State Geologist of North Carolina; C. W. Hayes, U. S. Geological Survey; J. C. Hartzell, South Carolina; F. A. [illegible], State Geologist of Pennsylvania; Lyman C. Josephs, of Newport, R. I.; Arthur Keith, U. S. Geological Survey; F. P. King, Portland, Maine; J. F. Kemp, Columbia University; E. C. E. Lord, U. S. Geological Survey; F. J. H. Merrill, State Museum, Albany; W. J. McGee, U. S. Bureau of Ethnology; S. W. McCallie, Assistant State Geologist of Georgia; E. B. Mathews, Johns Hopkins University; J. A. Mitchell, Mt. St. Mary's College, Maryland; F. H. Newell, U. S. Geological Survey; W. H. Niles, Massachusetts Institute of Technology; Edw. Orton, State Geologist of Ohio; C. C. O'Harra, Carthage College, Illinois; J. W. Powell, Bureau of American Ethnology; L. V. Pirsson, Yale University; F. B. Peck, Lafayette College; Heinrich Ries, Columbia University; Harry Fielding Reid, Johns Hopkins University; R. D. Salisbury, University of Chicago; A. C. Spencer, U. S. Geological Survey; T. W. Stanton, U. S. Geological Survey; G. O. Smith, U. S. Geological Survey; G. W. Stose, U. S. Geological Survey; J. Stanley-Brown, of Washington; G. B. Shattuck, Johns Hopkins University; Chas. R. Van Hise, University of Wisconsin; Lester F. Ward, U. S. Geological Survey; H. S. Williams, Yale University; C. D. Walcott, U. S. Geological Survey; T. G. White, Columbia University; I. C. White, Morgantown, W. Va.; Bailey Willis, U. S. Geological Survey; Elizabeth Kirkbride, Bryn Mawr College; A. B. Hoen, of Baltimore.

At the close of the visit of Sir Archibald to Baltimore on Monday, May 3d he passed several days in Washington. On Wednesday, May [illegible]th, he delivered an address before the Geological Society of Washington "Notes for a correlation between the Tertiary volcanic succession in northern Europe and in western America." After the lecture a reception was tendered to him by the members of the U. S. Geological Survey. Upon Thursday a visit was made to Bryn Mawr college, where an informal lecture on "Volcanic action in northwestern Europe" was delivered. Upon Friday a brief address was given to the American Philosophical Society in Philadelphia, and upon Saturday he arrived in New York. On Monday, May 10th, a lecture was given before the Brooklyn Institute. Several days were spent visiting relatives and friends in New England and he sailed from New York upon May 15th.

All those who participated in the excursions at the close of the lectures manifested great enthusiasm in all they saw and in the manner they were entertained. The following resolution was drawn up and signed by Sir Archibald Geikie and the entire company of geologists:

To the Board of Commissioners of the Maryland Geological Survey and the State Geologist.

GENTLEMEN: We have spent the last four days in a most interesting inspection of Maryland's geology, under conditions which your hospitable forethought has made peculiarly favorable. Our appreciation of the provisions made for the comfort of the party is keen, and our enjoyment of the excursion has been all that you could have wished it. In twice traversing the state of Maryland, opportunity has been afforded us of observing its mineral wealth, and in coming in contact with a wide range of phenomena illustrative of many of the aspects of geology. The field is rich, and it affords opportunities for inquiries which may add honor and wealth to the commonwealth, contributing at the same time to the sum of human knowledge. The exploitation of these opportunities will be safe under your guidance, and we wish you a speedy success.

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